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CEREMONIAL FRIENDSHIP AT ZUÑI

By ELSIE CLEWS PARSONS

THE Ashiwi or Zuñi are in the habit of applying their kinship terms loosely. They also describe many non-kinship relations in terms of kinship. Hence, although the Zuñi kinship term system is in itself fairly simple, the observer is at first bewildered, quite gratuitously bewildered, he in time concludes. One of the non-kinship relations which is described in terms of father and child is that between the initiate into the *kotikili*, the organization into which must enter every little boy and a few, a very few girls or women, and him who initiates. *ᴅachu*, father, and *atsiki*, boy, or *katsiki*, girl, are the conventional terms. I had put this detail aside as quite understood when one day I heard the middle-aged man whom my Zuñi hostess called her little Jim's ceremonial father, referred to not as *an ᴅachu*, the father, but as *an ᴘaᴘa*, the elder brother. "Why *an ʙaʙa*"? I asked. "Oh sometimes we call him *ʙaʙa*, sometimes, *ᴅachu*. You see he's also Jim's *kihe*." And then I learned that about two years ago, when Jim was eight years old, Kumaa, taking a great fancy to Jim, asked the consent of Jim's father to become *kihe* with Jim. Parental consent was asked in this case because of Jim's youth. Ordinarily the two *achikihepa* (*achi* is a dual prefix with reciprocal implication,[1]) are more of an age, nor need they ask familial consent. Jim's father agreed. At the *hilili* dance, Kumaa, who was one of the dancers, asked the *ne'wĕkwĕ*[1] while they were "playing" to get little Jim out in the center of *siaa tehwita*, the dance plaza, and give him the presents he, Kumaa, had for him—moccasins, cloth, and a silk kerchief such as the men wear around their heads. The following morning the *kuku*[2] of Kumaa came for Jim at his house, took him to their house and there washed his head in yucca suds.[3] They gave him the bowl he had been washed in, two other bowls, one big Navajo rug, and one blanket. Kumaa lives with his *an ᴅakwikwe*, his father's people,—his mother died when he was a baby—otherwise it would have been the duty of his *tsita*, mother, his *tsilu* or his *tsita tsanna* mother's elder or younger sister, to fetch and wash his *kihe*, Jim.

---

[1] Dr. A. L. Kroeber.

In the return ceremony in this case there was further irregularity. Jim's mother is alive, but she is a Cherokee and although she is sympathetic with Zuñi custom she holds somewhat aloof. So it was Jim's father's mother, his *wowa*, instead of his mother who when the time came, at the following summer solstice, fetched Kumaa to Jim's house. There Jim's father's father, his *nana*, brought his fraternity *mili*,[4] and said a long prayer, the chief purport of it being that now they would be all one and belong to the same family. Jim's father held Kumaa by the shoulders while Jim's *wowa*, his *kuku łashi*,[5] father's mother's sister, and his *kuku*, father's sister, rubbed Kumaa with meal and prayed. Then *an wowa* washed the hair, also the hands and face, of Kumaa. After that food was set before Kumaa and when he got up his presents were given him—a blanket, beads, one pan and two bowls, including as usual the bowl he was washed in.[6] Kumaa had now become Jim's older brother and Jim, his *sue* or younger brother. Hence-forward he would call Jim's parents *vachu* and *tsita*, and all Jim's other relations by the same terms Jim calls them.[1] And they in turn would call him by the usual reciprocal terms.

---

[1] A fraternity which shares in the "delight making" function of the *koyemshi*.

[2] Father's sister. The term is applied to the senior clans-women of your father's clan. It is used thus so inclusively that in references similar to those in the text the degree of blood kinship is generally difficult to ascertain.

[3] Hair washing is an accompaniment in Zuñi to all ceremonials.

[4] The decorated ear of corn and feathers every initiate receives.

[5] Old. *łashi* is also an age-class term applied to a man or woman past middle age.

[6] This bowl,—it is of a set design,—is always given too to the initiate into the *kotikili* and into the fraternities. It is used for general household purposes.

When the time came for Jim to be initiated into the *kotikili* there was none, his mother said, they wanted as much to be his initiator as Kumaa, and so Kumaa and Jim entered into this second relationship.[2]

Jim's father has also a *kihe.* Nine years ago, the first morning of the summer solstice celebration, this man's wife came to the Zuñi-Cherokee *ménage* I have been describing, saying to its male head; "I want you to come to my house."[3] Complying with the request,[4] the summoned man found he was to be "washed."

What the immediate exchange of presents was in this case I did not happen to inquire, but this particular "paling together" furnished illustrations of how the interchange of presents keeps up. Visiting one day in the house of her husband's *kihe*, my Cherokee hostess saw hanging on the wall a new small Navajo rug. "Has no one asked for it?"[5] she questioned. "No, some have talked about it, but no one has taken it down." "I will take it down," she said. On another occasion her husband himself cut off from his *kihe's* coat some silver buttons he coveted. When taking things in this way from your *kihe*, you are expected to have in mind something to give him in course of time. In spite of this reciprocity it is remarked that unless you are rich you cannot afford to have more than one *kihe.*

I was told of one case of a plurality. Suna, a man about forty, made a *kihe* about four years ago at the *kwamono* dance, a springtime *mukwe*[1] dance, of Samu, a man of his own age. Last spring at the *ʙasikiapa* dance Suna made another *kihe* of another male contemporary, Chani.

---

[1] This statement needs qualification. Kumaa calls the little girl, Jim calls his *kyauu*, elder sister, his *ikina*, younger sister (or brother).

[2] Another irregularity due to the anomalous mother. Customarily a boy's initiator into the *kotikili* is the husband of the woman who first touches him as he is born. To what extent the actual touching is make-believe I do not know, except that the woman doctor or midwife who actually makes the delivery does not count—today. (*Cf.* Stevenson, M. C., "The Zuñi Indians," *Twenty-third Annual Report Bureau of American Ethnology* (1901–2), p. 65.)

[3] The Zuñi house belongs to the wife.

[4] Asked in Zuñi to go to some one's house, although the reason for the request is never given, you never ask for it. Nor could you refuse or put off going. You just go.

[5] According to Zuñi etiquette you can not refuse to give whatever you are asked for.

The entertaining of this second relationship did not mean that the first had lapsed. Your *kihe* was your *kihe* for life, "a lifelong friend." Of course if he or she proved evil-speaking, or hateful, or bad, you were free, I was told, to drop him or her, but there was no ceremonial to dissolve the relationship and I heard of no case of dissolution.

The relationship could be made, as we have noted, between two men, it could also be made between a man and a woman or between two women. The latter combination seems rare. One old man had never known a pair of women *kihe*, another man knew only of one instance, the instance of his own daughter, a girl about fourteen, Tsaiaitatsisa, and an older girl whose father he knew by name but whose own name he did not know. "They washed each other," not at a dance, but in the summer time, Tsaiaitatsisa giving the other girl an *eha*, a woman's black blanket, and a belt, and the girl giving her also a belt and besides a *pitone*[2] and a buckskin for leggings.

Making *kihe* between a man and a woman seemed a much commoner occurrence than *kihe* making between two women, but less common than between two men.[3] The man and woman *kihe* would of course not marry, they would no more think of it than of marrying their brother or sister by blood or by clan. Nor would either marry into the family or clan of the other. My questions on this point of the possible marriage of *kihe* were received with the same incredulous amusement that had been provoked by questioning about marriage within the family or the clan.

---

[1] Moki or Hopi. Were you to call these pueblo kindred Hopi, a Zuñi would not know whom you meant.

[2] The square of cloth or silk a woman wears knotted in front and thrown across her back. She may not wear it indoors, but from the time she is quite a little thing, say seven or eight, she never leaves the house without it. Without the *pitone*, a Zuñi woman would say, she would feel "naked." Without it she would be subject, it is said, to improper addresses by men.

[3] I heard only of the *kihe* who were "personally known" to my informants. As for those in other circles I was told, "We don't know how they live; we are not on visiting terms." kwa kole adeǒne hon aiuyan̆nawomme kwa ashuwilaawanne
not how live we know not talk = to = have

Between one pair of *kihe* I heard about there could have been no thought of marriage on grounds quite other than kinship. About two years ago at a Navajo spring dance, Antonsi, a man about fifty, had "called out" Tsaniatsita, a woman "at least a hundred years old." There in the plaza he put a black dress on her, a silk *pitone*, a nice belt, and a white apron. He had said he was sorry for her, she was so old and poor with never anything nice to wear. The old woman lived with her daughter. Until of late years she had worked hard, like all Zuñi women, young or old. Little and bent as she was she would even be sent a long distance with a mutton on her back to trade with the Navajo. (She had been captured by the Navajo in her childhood). Then she had failed. Of no further use in the family, she was made to sit near the door at meal time, she was given what was left over, and she was poorly clothed—to the Zuñi such treatment of the very aged "is like putting away a piece of broken furniture."

The first pair of *kihe* in Zuñi were a young man and a girl, both at the time unmarried. The woman is dead, today the man is about sixty. He was an *upchianakwe*, and he himself called out[1] one of the girls in the *ikwiniakwe* (dance circle) at the *lewekwe* dance. He did this, he told me, for fun, stating that in this sport of his the *kihe* custom originated. It was out of his own head, he asserted, he had not heard of it from any one. At that time he had never visited the Hopi,[2] or been out of Zuñi.

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[1] In another instance a would-be *kihe* who was a *newĕkwĕ* also did the "calling out" at a dance. In the course of many Zuñi ceremonials the *newĕkwĕ* or the *koyemshi* "call out" a man and a woman (unconnected) to join in their "play," in a hopping match with them for example (men only), or in guessing what they have covered up. The couple receive each a present.

[2] I do not know whether or not an analogous custom exists among the Hopi, but among the Tewa of Hano, Barbara Freire-Marecco reports a term for chum, *pu'a, puwa* (*American Anthropologist*, (N. S.) 16, (1914) p. 270. Among the Tewa of the Rio Grande *k'ᴇmà* is the term in use for friend. It is *kⁱ'àbâ* at Jemez, *'anpuiwai* at Taos, *sâùke* at Santo Domingo. (Harrington, J. P., "Tewa Relationship Terms," p. 493. *American Anthropologist*, (N. S.) 14, (1912). I recall that on a ride among the

This social inventor is Dick, for some time lieutenant governor of Zuñi, the father of the aforesaid Tsaiaitatsisa. I talked with him after he had been mentioned to me as the originator of *kihe* by a much older man, a very reliable authority.

Dick himself has been a *shiwanni* or rain priest besides a member of the *lewekwe* fraternity.[1] In the other instances of *kihe* cited to me, sometimes the *kihe* belonged to fraternities, sometimes they did not. They belonged to the various clans. But I have no case, I notice, in which both *achikihepa* belonged to the same clan or to the same fraternity.

If the *kihe* relationship did originate with Dick[2] we are witness to the birth of a new custom, as rare an experience as catching sight of an autumn leaf the instant it leaves the twig. But the *kihe* phenomenon is possessed at any rate of unusual interest. Whether or not it originated spontaneously in a single mind or was suggested to that mind as an imitation either of a foreign custom or of an antique native custom, when Dick called out his prospective *kihe* at the dance he was institutionalizing friendship in Zuñi. That his sporting venture succeeded so well, that friendship was so quickly established as a Zuñi institution,[3] that the following generation was

---

Rio Grande pueblos a Santa Clara and I were referred by his kinswoman in Santo Domingo to her "friend," *amigo*, in San Felipe. Finding him we had but to mention Annunciata's name to be hospitably received. *Saukin* is the term for friend in Laguna and Acoma. At Laguna there is a ceremony of making friends in which hair-washing and present-giving occur, ceremonious acts suggestive of *kihe*-making at Zuñi, and as at Zuñi, the relationship may be entertained by two women or two men, or a man and a woman.

No *kihe* relationship has ever been entered into, I was assured, between a Zuñi and a person of another pueblo. There are now so many pueblo visitors from time to time at Zuñi that *kihe*-making with some of them, I surmise, will be in order. Recently in Acoma I saw a woman make a present of a bowl to a Zuñi visitor to whom she had also given a meal. "We are now friends," said the Acoma hostess.

[1] Neither of these social functions does he now exercise.

[2] That it was introduced by him I have no doubt, but that in his boyhood he had never heard of any analogous custom from visitors to Zuñi, from Laguna visitors perhaps, is a less settled point, a point waiting on further information about pueblo custom as well as on more intimate conversations with Dick or his contemporaries.

Dr. Kroeber suggests another possibility, namely, that the *kihe* custom was an ancient Zuñi custom *revived*.

[3] Jim's father, a man about twenty years younger than Dick, was thus unaware of the newness of the institution.

not even aware it was not an old custom, this successful establishment was due, I surmise, to certain conditions worth considering not only because they bear upon the *kihe* custom itself but because they may throw light upon the establishment of custom in general.

Dick's venture, startling as it is theoretically, and to one knowing the rigidity of social ties in Zuñi society it is startling, proceeded withal along conservative lines, along the same conservative lines that institutional friendship has followed in many other communities,[1] the lines of kinship. The new institution assimilated itself with the very old institution. Ceremonial friendship became merely a new kind of kinship or rather the same old kind of kinship once you accepted a necessary premise or two. Accepting that premise was made easy by a ceremony, just as ceremony ever makes any change easy and acceptable. In this case the ceremony itself was also familiar, it was a ceremony borrowed bodily from the ceremony of initiation into *kotikili* or fraternity. Thus institutional friendship was linked up by means of ceremony and terminology with both fraternity and family institutions. With the third great feature of the Zuñi social organization, the hierarchy, it was connected too because of "calling out" the *kihe* on sacred occasions, at "dances" or at the summer or winter solstice celebrations.[2] It gave no occasion to the hierarchy, as far as I can see, for antagonism. Nor did it prejudice against itself any economic interests. There are many communal aspects of Zuñi property-holding, and present-giving is a Zuñi habit. Present-giving between *achikihepa* was not startling. Like other forms of Zuñi present-giving, it brought, I incline to think, prestige.

---

[1] Parsons, Elsie Clews. "Friendship, A Social Category" in *The American Journal of Sociology*, September, 1915.

[2] Some day I hope to learn whether or not the *kihe* institution received no criticism from the Zuñi priesthoods. Covert as any innovation could well be, yet I would not be a bit surprised to learn that some of the old priests were at least critical of it. Extremely interesting in this connection is the *kihe* theory that existed in Zuñi, I am told, before Dick's venture, the theory that any one who was made a *kihe* by a *koko* (god) personator would die. That was the reason, my informant states, that although the Zuñi knew about the custom they made no use of it. Dick was the first to dare, "and you see his *kihe* died." This bit of history, if verified, might indicate that an old custom of *kihe*-making had been discouraged by the hierarchy.

Before concluding, I would draw attention again to the ***kihe***-ship between Antonsi and the poor old lady he took pity on. We see here, I believe, another invention, a new variety of the original invention. Tsaniatsita was a member of the same fraternity, the ***newěkwě***, as the old man who told me about her. She was too his step-mother, his ***inihau***, and she had brought him up.[1] Besides she belonged to his clan. But neither family, clan, or fraternity tie was a stimulus or a means to help out the old woman. Was she not living with her daughter, while her step-son lived with his wife and his wife's brothers? As for the fraternity they both belonged to, the mutual benefit features of Zuñi fraternities, if any, subsequent to initiation,[2] there be, I have yet to learn of. The idea that he or any other *outsider* should undertake to help Tsaniatsita seemed to me to give her fraternity fellow and her husband's son the same mild kind of amusement that any other unheard of custom aroused in him.

Under these circumstances may I hazard the suggestion that philanthropy in Zuñi has been the offspring of friendship as friendship has been the offspring of kinship, the other parent in each case, to continue, in Zuñi fashion, in terms of kinship, being an, in some way, exceptional individual mind or spirit?

New York City

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[1] Another case of a child of a widower staying with his father. Ordinarily the child would be brought up by his mother's people.

[2] The initiation may be so considered. The initiate is "given" to the fraternity in return for the aid of its members in his or her sickness.

Sometimes the cured one is given to the household of the medicine-man.

GAME TOTEMS AMONG THE NORTHEASTERN ALGONKIANS[1]

By FRANK G. SPECK

THE fundamental concepts of the totemic complex are being constantly revised by critics who are now devoting much attention to the uncertain phenomena of totemism as well as to the main theme itself. Indeed through the investigations of certain phenomena, which seem at first to be totemic only by allowance, results may be expected which will be useful in the process of interpretation. Accordingly in culture groups where organic totemism in its accepted sense is not a prominent feature of social life, where society seems to be loosely organized, we may look for some principles which may help us to understand what social factors are actually present when totemism in its more pronounced form is not.

In the region touched by the investigations to be briefly discussed in this paper, there are no evidences of a clan or gentile organization. The social unit appears to be what I have termed in general the territorial family group, an institution which is fundamental throughout the entire region occupied by the tribes of the Wabanaki group and the populations north of the St. Lawrence river. It may be advisable at this point to mention that Morgan, through not being intimate enough with the ethnology of this region and no doubt also through his eagerness to find social institutions corroborating his general hypothesis, mistook, I believe, the family groups with animal names for gentile groups. By disposing of this the only claim put forth to show the existence of the gens in the Northeast we find no other references in the early literature of the region to indicate seriously that any type of society did prevail here other than what modern investigation discloses. The family unit accordingly, as I have assumed in several previous papers from investigations in the social life of a number of northern tribes, is with little doubt not only the most ancient social grouping in this area but it was probably also the original characteristic one among the Algonkians.

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[1] Read before the American Anthropological Association, New York, 1916.

Since it seems that the weaker manifestations of the totemic concept are calling for more intensive study, as Dr. Boas himself points out in the latest word on this subject,[1] the individuality of the customs which form the totemic composite is often quite pronounced and the association of the elements themselves often quite artificial. Our data afford an instance of this. In our search for the separate historical or psychological sources which are now generally understood to compose the totemic complex, an interesting and moreover somewhat unusual series of cases is to be found among the Algonkians of the north and east. Among these tribes we can, I think, still perceive some of the elements which have contributed toward the social complex defined as totemic in the unrestricted sense of the term. The customs referring to diet-tabooes, naming, and religious practices which maintain an association with the totemic creature are quite common in various parts of the Algonkian group. But instances where the association is maintained to a noticeable extent by the *use* of the totemic creature are not so common.[2] In the region from which I have collected the material to be presented, the associated creatures appear in several tribes as economic totems in different degrees, association with which nicknames and paternal family names are determined, through which the use of land and game is inherited, and family kinship groups distinguished and held together by a common legendary origin.

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[1] F. Boas, "The Origin of Totemism," *American Anthropologist* (N. S.), vol. 18, no. 3, (1916).

[2] Observing in this region the attitude toward the totem where relationship is maintained by killing the totemic animal and eating it, and by using its fur, the association with the creature is not so unlike the more common practice of reverence for the totem by not killing, eating or using it as may seem. In the whole northern region the various animals are treated with respect by their slayers who prepare the bones and other remains of the game and dispose of them in a manner thought to be satisfactory to the animals' souls, so to speak. Thus in the religious thought of the north a hunter is satisfying an animal's spirit about as much when he kills the animal and disposes properly of its remains as when he refuses to molest it at all. While the above procedure is quite contrary in practice to the procedure of African, Australian, and a number of American totemites, it is not so contradictory in principle, its objective in reality being the maintenance of the game.

MALECITE

Among the Malecite the indications point out most clearly that name association with certain animals which constitute the food supply of either individuals or of larger groups, determines individual and even family group naming. Several quotations from older accounts show that the tribe itself was known by the nickname Muskrat, partly from depending for subsistence upon the muskrat.

> There was also on the St. John, a division called Warastegoniak,[1] who were subsequently called by the other Abenaki the Mouskouasoaks, or water-rats either because, like these animals, they lived on the banks of the river, or because they highly esteemed the muskrat as food, which they do at the present time preferring its flesh beyond that of any other.[2]·

The Abenaki (Aroosagunticook and Norridgewock) of St. Francis, Pierreville, Quebec, also designate the Malecite as *Muskwa's·uwak*, Muskrats, to this day. A synonym for the Malecite, among the Penobscot at Oldtown, Maine, is also *Muskwe·'s·uwαk*, Muskrats, and among the Micmac *ki·u'sux*, Muskrats.[3]

Among the Malecite in general, personal nicknames are in addition frequently determined by the kind of game which a hunter may pursue habitually, or that which he may happen, in the fur quest, to be most successful in getting. To illustrate this point I offer a few personal nicknames from the village of Tobique.[4]

pagα'm‘k‘	fisher (Mustela pennanti)	Joe Nicholas
ki·'uni·'k‘	otter	Sosep Tomah
kwa'k‘ʷsɛs	fox	Chas. Nicholas
sku‘'tαmuk‘	trout	Andrew Nicholas
espα'ns	raccoon	Frank Francis
keni·be'k‘sis	(?)	Francis Lola

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[1] The native designation of the Malecite is *Wula'stegwi·ak*, Good River People.

[2] E. Jack, Maliseet Legends, *Journal of American Folk-Lore*, vol. VIII (1895), pp. 193, 201, 207. Jack also says that the *totem* of the Malecite is a muskrat.

[3] See, also, S. T. Rand, *Legends of the Micmacs*, p. 34.

[4] This material was first brought to my attention by Louis Francis and his wife Caroline Francis, in 1913, at Bar Harbor, Me., both of them reliable informants of the old school. The fragmentariness of this material, however, is due to the fact that I was not aware of the prospective development of the topic at the time.

Here we also have family nicknames of the same general scope and derivation.

Native Family Nickname		*Christian Family Name*
niˑmo'ks	sable, marten	Sockobi
abi'tciˑlu	skunk	Saulis
ma'lsɛm	wolf	Sockabesin
skuʻ'tαmʻ	trout	Ketcham
sαgwe'sˑ	weasel	Mitchell
kiˑu'χɛs	muskrat	Saulis
gwa'biˑtʻ	beaver	Sabadis
tciˑaʻ'kes	mink	Paul

The diminutive plurals of these animal names, ending in *-siˑzak* were given by the informants as being commonly applied in the village to the younger members of some of these families, and the augmentative *ktciˑ*, "old, big," as applicable to the older men.

In the case of the Malecite the game totem idea does not, so far as I know, exclusively dominate family or individual psycho-social associations, but it pervades them deeply in practice, because these animal nicknames have become to a certain extent incipient group names.

Tribal designations in which an animal figured as the emblem, existed apart from the family totem among other of the eastern tribes. In such cases, again, as among the Malecite, the animal was the game animal. The family emblem and the broader local band or tribal emblem, while probably related phylogenetically, need not, nevertheless, be confused in interpretation. Tribes, as well as local bands composed of groups of families, had the totemic symbols, but unfortunately we do not know much about the possible ideas associated with them since our knowledge of them comes only from some early writers. Mallery[1] presents a little information on this subject obtained in 1888 from a Passamaquoddy. He says:

> The animals figured are in all cases repeated consistently by each one of the several delineators, and in all cases there is some device to show a difference between the four canoes (figured with the animal representations) either in their structure or in their mode of propulsion, but these devices are not always consistent. It

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[1] Garrick Mallery, "Picture Writing of the American Indians." *Tenth Annual Report, Bureau of American Ethnology*, 1888–9, pp. 378–9.

is therefore probable that the several animals designated constitute the true and ancient totemic emblems, and that the accompaniment of the canoes is a modern differentiation.

The tribal emblem of the Passamaquoddy consists of two Indians in a canoe, both using paddles and not poles, following a fish, the pollock.[1] The emblem of the Malecite is two Indians in a canoe both with poles, following a muskrat. The Micmac emblem is two Indians both with paddles, in a canoe built with high middle parts, "familiarly called humpback," following a deer. The Penobscot emblem is two Indians in a canoe, one with a paddle and the other with a pole, following an otter.

PENOBSCOT

The Penobscot were divided into twenty-two individual family groups, not exogamic, with an indeterminate line of descent, some of them with a legendary origin and possessing names which are names of animals with which they claim indirect relationship. The family forms also a local band, with preempted hunting territory and emblems, used as territory markers, consisting of the figures of the associated animals. Our point here, however, is that the animals with which the families claim their associations are the animals which these same families prey upon, obtaining their flesh and hides for subsistence. The family names in some instances are thus derived from the animals which the members mainly hunt and which are thought to abound over others in their particular hunting territories. So we have the maritime families subsisting primarily upon the marine animals and deriving their group names and individual names from them. These names are, moreover, inherited by the men through their fathers. While not all of the family groups fall under the above game-totem classification, thirteen of the approximate twenty-two do. In these instances we should note that the informant, Newell Lyon, stated distinctly that the different families subsisted largely upon the flesh and traded in the hides of the animals by whose names they were known.

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[1] The Passamaquoddy tribal name is derived, according to numerous authorities, from the term denoting "those who pursue the pollock." It might appear from the above statement, which I have not as yet had the opportunity to test among the Passamaquoddy, that the tribal name, as in the case of the Malecite just mentioned, is derived here also from the animal mostly sought for by the people for food.

Still another instance of the same thing is to be found in the case of the band of Indians who inhabited Cobbosecontee lake, Kennebec county, Maine, who were known as *Kaba''sekantiak*, "people who pursue the sturgeon" (*kaba''se*). (*Cf.* E. A. Kendal, *Travels through the northern parts of the United States*. New York, 1809, p. 124). These Indians not only regarded themselves as Sturgeons, but had a legend relating how one of their ancestors was transformed into a sturgeon. In a similar category perhaps should be included Le Clercq's information (quoted by Mr. Mechling, *American Anthropologist*, (N. S.) vol. 18, 1916, p. 300) that the Micmac at Restigouche wore a salmon, those of Miramichi belonged to a band which had an emblem similar to a cross, and that the other bands had their particular emblems.

Totemic Name	*Modern Name*	*Location of Hunting Territory*
Lobster	Mitchell	Penobscot bay.
Crab	Susup	Penobscot bay.
Eel	Neptune	Kenduskeag river valley (*Kαnde's·ki·k* "overgrown eel place").
Beaver	No modern name	Penobscot valley.
Sturgeon	Sockalexis	Ragged lake, and Moosehead lake.
Raccoon	No modern name	Salmon river region.
Wolf	Polis and Susup	Nahmakanta region.
Squirrel	Attean	Caribou and Ragged lakes region.
Wolverine	Lewis	Mattawamkeag region. (*L'αnksus* "wolverine" lake).
Otter	Saul and Nicola	Katahdin region.
Wildcat	Fransway and Penus	Musungum and Seboois lakes region.
Hare	Newell	Masardis waters
Yellow Perch	Penewit	Caucomgomoc region.

The economic aspect in the associations of the families listed is as strong from the internal point of view as it is in the direct testimony of the informant. I might add that the Penobscot still apply animal temporary nicknames to individuals, names derived from any noteworthy fondness either for hunting, trapping, or eating certain animals. Trivial nicknames also arise from having had a successful season in hunting some particular class of fur-bearing animals. While hunting or trapping a certain animal, an unusual experience, an extraordinarily piece of good or bad luck, an escape, an unexpected happening, noteworthy weather and the like, are often sufficient to give rise to nicknames from the particular animal concerned. So, as among the Malecite, there are always apt to be at Oldtown men known as Muskrat, Weasel, Chickadee, Moosemeat, Bear, Smoked Fish, and others whose names are accounted for in the village by current anecdotes.[1]

An important psychological association incidental to the matters presented here is a certain function of another kind of individual totem as a dream animal and as a supernatural helper, a sort of animal proxy in whose disguise the individual may transfer his soul in order to fight his enemies or project himself into other places for spying. The totem animal is known in this respect as *baohi·'gan*, "means by which magic is performed." Many tales are told of fights between the proxies of different sorcerers in the past. I shall not, however, attempt to deal in further detail with this phenomenon now. The concept is shared by all the Wabanaki tribes and the term *bao-* is cognate in all these dialects in the same sense. I may say that in the list of families with totemic names which has just been given, the animals designated were not automatically the *baohi·'gan* of the men of these families; the latter was a different animal associate. The above conclusions are based on conditions existing only in the memory of the oldest men now living among the Indians.

MISTASSINI AND MONTAGNAIS

Differing considerably in its scope, a similar psychological phenomenon appears in the association that prevails between a hunter and his dream manitou among the Mistassini band of Labrador. We may, I think, perceive a remote similarity here with what has already been discussed from the corresponding area south St. Lawrence. I will give the substance of information obtained from a Mistassini informant just as the data appear in my notes, because at the time of my investigation I was not fully aware of the import of the data and I have not had the opportunity of returning to this field since.

The Mastassini,[2] I may say, neither derive personal nicknames, nor family names nor band names from the animals hunted. The individual families, to be sure, do form fairly definite groupings with paternally inherited hunting territories but no further associations have as yet been noted as existing between the individuals and animals. Hence, group totemism has not developed here as far as it has among the Penobscot although the family territorial group seems to be the fundamental social unit to as great a degree.

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[1] A census of nicknames and explanations from the tribe is in course of preparation in another paper dealing in detail with the family grouping in this tribe.

[2] The members of this band, who call themselves *Mictaci·ni·wi·i·'nuts*, "big rock people," hunt about Lake Mistassini. They form one of the intermediate Cree-Montagnais groups.

The following translation of an excerpt from a text will serve to introduce and define, though only roughly, an association between the hunter and his dream spirit as a game animal.

> It is known that when a beaver once comes to tell something it is never in vain that he speaks, for it is like what a great prophet says. He also wants his meat pan made to look pretty. After it is done and decorated just as he wants it, he never fails (to supply his beneficiary with instructions where to find his game). Yet he does not wish different kinds of meat to be cooked in his meat pan, only his own kind of meat he wants to have eaten in it.[1]

By this we understand that the hunter is visited by a guardian spirit in the shape of a beaver who instructs him where and under what conditions he will find game. Since beaver forms such an important element in the diet of the tribes of this region, the natives believe that the beaver spirit in dreams directs them generally to where beaver may be found. The meat pan referred to is a flat birchbark dish, about eighteen inches in diameter, carefully sewed, and decorated with spots of red paint in the bottom both inside and out and with a covering of red paint over the seams. Whatever meat is obtained, after following instructions received in a dream given by the beaver guardian spirit, is to be eaten in this pan without the use of a knife. The game so obtained must be cooked without the backbone. It must be boiled whole and it must be consumed before the following morning, at which time, it is believed, the beaver spirit will again visit the hunter and give him new instructions for his hunt. The meat pan referred to is an article found in the possession of almost every hunter and family-head among the Mistassini. It is kept carefully wrapped in a white cloth when not in use. Kakwa, the informant, said that he understood that good hunters would attribute their success to having received dream directions almost every night.

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[1] Narrated by Kakwa at Pointe Bleue, Province of Quebec.

Again, at Lake St. John, among the related Montagnais, we find a similar custom. Here, in brief, the dream usually consists of the vision of almost any of the numerous game animals. This dream is thought to anticipate the capture of the animal seen. Here also each hunter has his own pan from which he eats the meat that he obtains. The meat pans of the Lake St. John people are somewhat different from those of the Mistassini in being deeper: on the outside are etched the figures of animals which have revealed themselves to the owner in dreams and which have been taken.

In both of the above cases, the point to be emphasized in connection with the game totem idea is that the personal guardian spirit of the individual is an animal of the same class that is hunted.

We have seen that the idea of the so-called game totem is fundamental to the social life of the Penobscot to a degree that is not equalled in the related neighboring tribes. This appearance of greater prominence among the Penobscot, it cannot be denied, may possibly be due to the inequality of the methods of inquiry. Yet I may say that I have not been dependent solely upon the negative character of the evidence presented by Wallis and Mechling, who have both orally reported on the question of totemic groups in these tribes, because I have myself tried informants among the Malecite and several Micmac bands without disclosing the material that is so strongly featured among the Penobscot. I believe that the differences in social life among these tribes, differences indicated by our available material, are fairly real ones and that they are to be accounted for by the assumption that the tribal groups were developing socially along independent lines. Considering the restricted area involved and the differences encountered, one is impressed, incidentally, by the necessity of following lines of independent inquiry in investigating this field.

Whether we are to regard the totemic complex as a natural but unconscious native concept, or whether it is an artificial complex, it seems apparent that the multi-variety of the associated forces in inchoate stages of society is shown here in a single primitive culture area, where we see instances of the elementary individual manitou, the naming, the mythological, symbolic, and pseudo-physical characteristics, all, in nascent aspects, derived from or associated with the animal hunted for food. The game totem idea appears plurally in connection with some of the above aspects of society in this region, which incidentally in several of the general totemic origin theories, Haddon's in particular, have figured as possible starting points.

It will be very instructive if, in the prosecution of further studies, any instance among the Algonkian tribes comes to light where the territorial family group with indeterminate descent has metamorphosed into a true matrilineal or patrilineal group. I think that I am not entirely wrong in believing that the Penobscot present an incomplete phase of such a development trending toward the gentile formation. This, I also think, is the normal direction of social growth among the Algonkian, where they are unaffected by outside contact, a claim which is supported, as far as present investigations show, by the wide distribution of the territorial family band with patrilineal features in the typical Algonkian culture-habitat. It is quite consistent that the more southern Algonkian tribes, like the Delaware, should have the clan organization superimposed upon the older formation, for among them agriculture is in evidence proportionate to their southern habitat and, perhaps still more significant, in proportion to their proximity to the Iroquois.

As an excuse for presenting at this time such a very brief and superficial outline of a somewhat heterodoxical form of the totemistic association, I should perhaps say that certain circumstances seem to stand in the way of the publication of my more detailed studies of northeastern Algonkian ethnology. Since it may be some time before these do appear it seems desirable that the topic at least be made available.

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VARIATIONS IN THE GLENOID FOSSAE[1]

BY LOUIS R. SULLIVAN

THE glenoid fossae are depressions in the base of the squamous portion of the temporal bones just posterior to the roots of the zygomae. They are usually described as being deep.

W. L. H. Duckworth[2] in his description of the crania of aboriginal Australians remarks on the extremely flattened and shallow glenoid fossae and submits that this is an approach to a state that is usual in anthropoid apes. In a later work[3] he lists this condition as a characteristic of both Australians and Tasmanians.

In 1915 Mr. F. H. S. Knowles published a paper on the "Glenoid Fossa in the Skull of the Eskimo"[4] in which he represents the Eskimo as "the champions of the shallow glenoid fossae." He explains this shallow condition by the tough nature of their food and the great development of the lower jaw and masticatory muscles. His observations were on Eskimo crania from Smith Sound.

In the July–September number of the *American Anthropologist*, 1916, Mr. Hawkes and Mr. Wallis confirmed Mr. Knowles' observations to a certain extent, but pointed out that there was considerable variation even among the Eskimo.

The important question raised by these papers is the cause of this structural variation. Is it a racial characteristic linking those individuals or races which possess it to the apes or is it a caenotelic character recently acquired by the race or by the individual as an adaptation to environment, food habits, and methods of mastication?

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[1] Preliminary report read before American Anthropological Association, New York, 1916.

[2] *Studies in Anthropology*, p. 107.

[3] *Morphology and Anthropology.*

[4] *Anthropological Series*, No. 4, *Museum Bulletin*, No. 9, *Canadian Geological Survey.*

My observations on the collection of skulls in the American Museum of Natural History agree in the main with those already mentioned. There are a few points I would like to add to and one or two new observations.

I believe I am safe in making the following general assumptions. If this shallow glenoid fossa is a racial characteristic it should be relatively constant especially in such a comparatively pure race as the Eskimo. If it is of functional origin we should expect it to be extremely variable.

Mr. Knowles does not state the number of skulls in the series on which he based his conclusion. In a series of twenty-seven Eskimo skulls from Point Barrow, Alaska, Mr. Hawkes and Mr. Wallis found twelve shallow, twelve medium, and three deep. By "shallow" we mean that the articular surface has apparently moved forward so that the greatest pressure is brought to bear on the root of the zygomae or anterior articular eminence which also increases the antero-posterior diameters of the fossae. I have separated the shallow from the extremely flattened and get the following results:

No. in Series	*Region*	*Deep and Short*	*Medium and Short*	*Shallow and Elongated*	*Flat and Elongated*
15	Isle of St. Lawrence, Alaska	0	5	6	4
33	Indian Point, Siberia	7	6	13	7
50	Point Barrow, Alaska	10	5	14	21
19	Ponds Inlet	9	2	1	7
5	Smith Sound	1	0	3	1
122		27	18	37	40
	Approximate percents.	21	15	31	33

These figures would tend to diminish the value of the glenoid fossa as a racial characteristic if we consider the Eskimo as a unit. The results do not detract from Mr. Knowles's explanation of the cause.

I would also like to call attention to the fact that shallow glenoid fossae occur in other races with a similar frequency. The results in the accompanying table are largely from American Indians.

Smaller series of four or five crania from other parts of the United States, Solomon Islands, Australia, American Negroes, gave similar results. According to Mr. Duckworth it must be of frequent occurrence in Australians and Tasmanians. Giuffrida-Ruggeri[1] finds this characteristic in Italian crania. Mr. Hawkes in unpublished data has shown considerable variation even in the apes. All of which would seem to indicate that it is a widely distributed structure.

No. in Series	*Region*	*Deep and Short*	*Medium and Short*	*Shallow and Elongated*	*Flat and Elongated*
86	Utah, Grand Gulch	16	32	28	10
4	Arizona	1	1	1	1
8	Colorado	1	2	4	1
10	New Mexico, Zuni	2	1	0	7
91	Mexico, Valley of Mexico City	22	22	18	29
12	Mexico, Valley of Mexico City (trepanned)	2	6	3	1
19	Mexico, Chihuahua	6	6	6	1
17	Mexico, Cora	3	3	7	4
56	Huichol	5	15	15	21
92	Tarasco	17	41	19	15
54	Peru (mountains)	0	26	17	11
150	Bolivia, Huata (Deformed)	42	38	50	20
268	Bolivia, Sicasica (Deformed)	91	37	86	54
11	Patagonia	0	0	10	1
14	Bedouin	7	4	3	0
26	Samaritan	8	7	9	2

I should indicate that my determination of the depth of the fossae was based on arbitrary judgment. This method was adopted after a careful consideration of the method proposed by Mr. Knowles, the results of which do not seem to justify the labor. I was also influenced by the assumption that my error would be diminished by the examination of larger series.

So far I have considered the fossae to be identical on the left and right sides. But in all series examined I noted a marked asymmetry of the fossae not only in different skulls of the same race but also on the left and right sides of the same skull.

With this asymmetry of the fossae goes an unequal wear of the teeth, the teeth on the side of the more shallow fossae being worn more than those on the opposite side. In many cases where an asymmetry of the fossae was found it was impossible to make the comparison on account of the loss of teeth. In the few cases where the teeth were present in both upper and lower jaw the wear justified the conclusion that the difference in the shallowness of the sides was due to a greater use of that side which was most shallow. In some cases I also found an asymmetry in the form of the condylar process of the mandible.

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[1] *Science*, N. S., vol. XLIII, 1916, pp. 904.

Of the ten Eskimo skulls from St. Lawrence Island examined for this characteristic I found four which showed asymmetry; twenty skulls from Indian Point, Siberia, showed five asymmetrical; twenty-eight Peruvian showed eight asymmetrical. I also found marked examples in smaller series from Zuñi, New Mexico; Sicasica, Patagonia, Australia, Solomon Islands, in the crania of Indians from New York and Tennessee, and in those of American-African Negroes. I believe this is strong evidence for the functional modification of the fossae. It also indicates that individual habits of mastication influence the form of the articular surface.

The embryology, structure, and movements of the joint should also be emphasized. They are decidedly in favor of a functional modification. At birth in man and apes at least, and as far as I have observed in other mammals, the fossa is shallow or flat. In man it remains flat until about the age of three.[1] The region anterior to the Glasserian fissure is extremely thin. Even in the adult it will transmit light in some cases. It seems reasonable to believe that such a structure would be depressed by pressure. In accounting for the shallow fossa the point is that the greatest pressure is not brought to bear on this region, but is distributed over a larger area, chiefly anterior to the typical position of the glenoid fossa.

This is a double joint.[2] It is divided into two parts by an interarticular disc of fibro-cartilage. The hinge-like movements of the lower jaw take place between the jaw and this cartilaginous disc. The external pterygoid muscle is attached in part to the disc and consequently the disc and the condyle of the lower jaw move together on the articular surface of the temporal bone in gliding and rotatory movements.

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[1] *Human Anatomy*, Piersol.

[2] *Human Anatomy*, Gray.

The cartilaginous disc helps to break the force of sudden shocks, but I believe in the gliding movement considerable pressure is brought to bear on the articular eminence. The disc is very thin near its center and shows the probable effect of wear by being frequently perforated.

Finally, I believe we are justified in concluding that the variations in the glenoid fossae are due to function. That the rotary, and fore and aft movements of the jaw in mastication tend to move the articulation forward on the roots of zygomatic arches and wear down the articular eminence. Also that the nature of the food influences the masticatory habits of the individual.

If we are correct in this conclusion, the shallow glenoid fossa can have no racial or phylogenetic value. At best, it might give a possible hint as to the masticatory habits of an individual.

Doubtless other considerations such as age, growth, and pathological conditions enter into this matter. I should also expect to find many other changes accompanying this variation.

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THE PLACE OF COILED WARE IN SOUTHWESTERN POTTERY[1]

By EARL H. MORRIS

THE acceptance of the evolutionary theory that basketry is the necessary precursor of clay vessels has given rise to a general misunderstanding concerning the place of coiled ware among the pottery types of the Southwest. A superficial examination of this coiled pottery tends to establish the following points in the mind of the observer: (1) the small range of form to which coiled ware vessels are restricted; (2) the uniformity of pattern produced by the coiling; (3) the poor quality of the paste, and the crudeness of finish displayed by the vessels; (4) the close association between technique and function, the coiled pots being used almost without exception as cooking utensils.

These four features tend to substantiate the belief that coiled ware is the oldest variety of Southwestern pottery, and the prototype of smooth and painted ware. However, theoretical considerations alone are sufficient to submit such an assumption to serious doubt. The subtle discriminations which arise from a long and careful study of coiled ware reveal a multiplicity of patterns, an inherent beauty of design, and a masterly command of technique which necessitate the conclusion that greater skill would be required to produce this type of vessel than would suffice to form and smooth the finest specimens of Southwestern polished ware. In shaping the sides of a smooth jar, the gourd-shell or polishing stone might be used to obscure irregularities of surface; and to equalize uneven thickness of the vessel walls, a bit of clay might be scraped off here or added there with no damage to the finished product. But to lay spiral coil upon spiral coil in uniform thickness, to regulate the length of the coils so as to produce the desired slope of the vessel walls, and at the same time to make each pressure of the thumb mold its minute portion of the design which is worked out in the coiling, demands an initial accuracy of movement, and a control of a complicated technique worthy of an artist, and not at all compatible with the bungling skill of a beginner.

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[1] Read before the American Anthropological Association, New York, 1916.

In spite of the force of this evidence, one hesitates to take issue against an established theory without empirical data to serve as a basis for one's contentions. Fortunately this data has been brought to light by excavations made in the valley of the La Plata river, San Juan county, New Mexico.

At one of the sites examined, there were found the clearly stratified remains of three periods of occupation. The basis of distinction between these periods rests upon a sharp differentiation of architectural and ceramic types, but for the purposes of this paper, little attention will be paid to anything but the cooking vessels of the successive periods, since with few sporadic exceptions vessels with unobliterated coils were used about the fire.

In tracing the development of coiled ware through the different periods, three things will be considered: (1) the form of the vessels upon which the coils are to be found; (2) the portion of the vessels upon which unobliterated coils occur; (3) the nature of the coils themselves.

The cooking pots of the first or most ancient period consist of globular or sub-spherical bodies surmounted by straight or slightly recurved necks or upper zones. In the majority of instances both the bodies and the upper zones present a smooth, though unpolished exterior. Perhaps thirty percent of the vessels have upper zones composed of from four to eight broad heavy bands of clay between which the lines of union have not been entirely obscured. There are few examples in which the bands were applied spirally, the most common method having been to add one complete ring or coil above another. The bands are irregular, and roughly finished, presenting low relief in cross section.

The chief difference in form of the vessels of the second period is a tendency to slope inward from the point of greatest diameter, with a consequent lessening of the proportionate diameter of the mouth. Apparently without exception, the cooking pots of this period have plain unpolished bodies, and coiled upper zones. The coils were applied spirally, and as contrasted with those of the earlier period, are much narrower, and of uniform width. They present high relief in cross section, the deep grooves between the coils seeming to have been accentuated by drawing a bone awl or other pointed instrument along the indentations left when crimping the strips of clay together.

The typical form of the vessels of the third period is that of an egg resting upon the large end, with the small end truncated and surmounted by a short, abruptly recurved neck or rim. The entire exterior of the vessels is covered with a spiral coil which begins at the bottom and proceeds continuously to the rim. The coils are characteristically narrow, and vary from high to low relief in cross section. This period may be called the Golden Age of coiled ware. There is a surprising variety of pattern resulting from the method of manipulating the coils, and instances are fairly numerous of decorations incised upon the coils, with occasionally bands and spiral fillets of clay added to produce pleasing patterns in relief.

We have, then, during these three periods a progressive change of form from globular or sub-spherical bodies with relatively straight upper zones, to egg-shaped bodies with short abruptly recurved necks. This in itself proves nothing but a constant tendency in one direction which may be considered to have corroborative significance when considered together with the other facts in the case.

Unobliterated coils appear upon the upper zones of less than half of the cooking pots of the first period, upon the upper zones of all of those of the second, and upon the entire exterior of those of the third period. The individual coils of the first are broad, clumsy, and crudely applied. Those of the second are narrower, more regular, and more skillfully applied, while those of the third period, both from the point of view of variety, and that of skillful manipulation, reveal a mastery of the technique which during the earlier periods was undergoing gradual elaboration.

Having thus pointed out the general development of coiled ware, it is of interest to determine where the periods during which it took place fit into the ceramic sequence of the parts of the Southwest for which this sequence has been determined. The ceramic series for the Rio Grande valley has been determined by Mr. Nelson and Dr. Kidder, and it is probable that the sequence outlined by the present writer for the upper San Juan valley will be verified by future research. According to these series, Pueblo culture, at least from the point of view of ceramics, became extinct in the upper San Juan valley approximately at the time when the occupation of the Tano sites examined by Mr. Nelson began.

In the chronological series, the following types may be recognized:

1. Pre-Pueblo
2. Early black-on-white
3. Late black-on-white
4. Black-on-red and black-on-yellow
5. Two-color glazed wares
6. Three-color glazed and painted wares
7. Historic two-color glazed wares
8. Modern painted wares

Here the first type recognized by Mr. Nelson has been changed somewhat, his sub-types having been raised to the rank of types, but without altering the sequence.

But to return to our original consideration. The strata of the refuse piles from which the cooking vessels of the first type, that is, those of which some thirty percent have upper zones composed of broad heavy bands of clay, were taken, accumulated during pre-Pueblo time.

The strata containing cooking vessels of the second type, characterized by upper zones composed of narrow spiral coils in high relief, pertain to the early black-on-white period. In the upper levels of the refuse piles, the coiled ware showing the greatest variety of finish, and the highest development of technique was correlated with painted ware of the late black-on-white type.

It was, then, during late black-on-white time that coiled ware reached the zenith of its development. Toward the close of that period, or at the beginning of the next, which is characterized by black designs upon a red or yellow ground, slight decadence took place. This decadence is to be observed in the coiled ware from the lowest strata of the Rio Grande refuse heaps examined by Mr. Nelson.

The smooth ware associated with the cooking pots of the first period is well made and bears elaborate painted ornamentation. Thus it appears that painted ware was highly developed when coiled ware was still crude and quite rudimentary. This, together with the fact that from pre-Pueblo to late black-on-white time, coiled ware constitutes a rapidly ascending series, seems to justify the conclusion that were it possible to trace it back to still more ancient times, it would become less and less frequent, and finally go out entirely, and that at a time when smooth ware had made appreciable advancement.

In the light of present knowledge it may be said that coiled ware originated, perhaps as a variant or offshoot of smooth ware, during the pre-Pueblo period, and reached its culmination in the late black-on-white period. At, or shortly subsequent to the close of that period, coiled ware passed through a short interval of deterioration. Thereafter it continued with practically no change into historic time, and ceased to be manufactured about 1680.

All obtainable evidence confirms the belief that smooth ware was the first type of pottery made in the Southwest. It existed side by side with coiled ware through the entire life cycle of the latter, and continues to be made by the Pueblo potters.

There is nothing in the above to discredit the possibility that the coiling process was the first to be used in the Southwest. It is the belief of the present writer that such was the case, but only a more detailed study than has yet been made of the structure of pre-Pueblo pottery can settle the question.

The fact of importance is that the variety of coiled ware which has generally been accepted as the most ancient of Southwestern pottery, is shown to be the product of the culmination, and not of the beginning, of an intricate and highly elaborate technique. Moreover, these facts have a vital bearing upon the relation of ceramics to basketry.

Until there was stratigraphic evidence to the contrary, it was permissible, though not justifiable when relative difficulty of construction was considered, to postulate a genetic relationship between coiled baskets and the coiled pottery of the Southwest. The spiral coils of clay were the analogues of the spiral coils of baskets, and the indentations made by the thumb closely correspond to the irregularities of surface produced by the intertwining of the strips which pass at right angles to the coils. It was supposed that in a change of materials from rolls of fibre and withes, to clay, as many motor correlations, and as much of the established technique as possible, were carried over to the new art. However, stratigraphy has shown that coiled ware most closely resembling basketry made its appearance long after Southwestern tribes had learned to make vessels of clay. Hence it appears that proofs based upon coiled ware mean nothing at all, and other arguments must be brought forward if the theory that the pottery of the Southwest is a direct outgrowth of basketry is to be substantiated.

NEW YORK CITY.

A PREHISTORIC WIND-INSTRUMENT FROM PECOS, NEW MEXICO

By CHARLES PEABODY

IN July, 1916, my daughter Caryl and I had the pleasure of a two-weeks' visit to the Phillips Academy, Andover, excavations at Pecos, New Mexico, conducted by Dr. A. V. Kidder of Cambridge. It is by his permission that I have had the chance of experimenting with and describing the little instrument in question.

During our visit, Dr. Kidder's assistant, Mr. C. E. Guthe, was engaged in clearing the northwestern part of the mesa outside of the north building of the Pueblo, but inside the "peribolos" wall; here were many burials of early prehistoric date; with skeleton No. 655, at a depth of about one meter lay a small bone instrument; its archaeological date is "Agua Fria" or "Frijolito," *i. e.*, in the second or third of the six prehistoric strata, counting upward, into which Mr. Kidder divides the successive deposits.

The specimen (fig. 1) is made from the wing-bone of a bird about the size of a turkey, and has been cut squarely off at both ends, rubbed and smoothed on the exterior, and pierced by four holes.

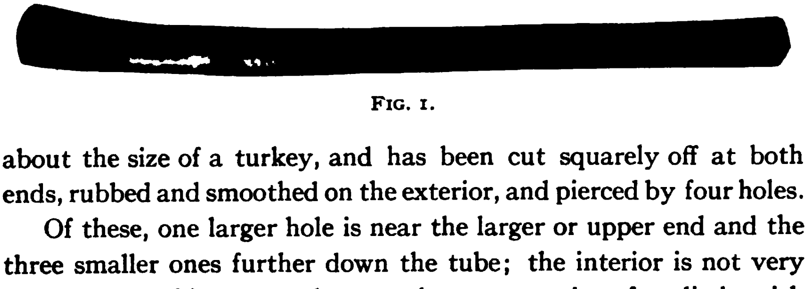

FIG. 1.

Of these, one larger hole is near the larger or upper end and the three smaller ones further down the tube; the interior is not very smooth, but this, strangely enough, seems to interfere little with its virtues as an instrument. Near the lower end are a few transverse scratches, as if the maker had tried to cut the bone higher up.

Both ends are open and there is no trace of gum, bitumen or resin from which the presence of a stopper or of a diaphragm may be inferred.

The following measurements are given for comparison with those taken on my own flute (made in Paris about 1890):

	Pecos M.	Paris M.
Length over all	.185	.710
Upper end to first finger-hole	.105	.300
Upper end to upper edge of mouth-hole	.037	.070
Length of mouth-hole	.008	.012
Width of mouth-hole	.006	.010
Diameter of lower end	.010	.016

It will be seen that in the modern flute the mouth-hole is proportionally nearer the upper end; the finger-holes are further down the tube if the modern flute is imagined as cut off below the upper three finger keys (below G-natural).

The instrument, blown into or upon as found, will produce a certain number of more or less interrelated notes, some of them excessively shrill; only with the upper end closed, however, have I been able to play it as a musical instrument. I inserted about two millimeters' thickness of modelling clay and closed the upper end tight and then used the tube as an ordinary flute.

Beginning with all the holes covered and raising the fingers one by one, we obtain: C sharp (fig. 2), D sharp, E sharp, the open tube gives, F sharp, and easy harmonics give G sharp and A natural; half-stopping the holes will produce D natural and E natural; but I have not yet succeeded in making it play G natural. Its range therefore is a minor sixth, omitting the diminished fifth. If the flute is well played its tone is singularly sweet; it is somewhat different from that produced on modern piccoli of wood or metal.

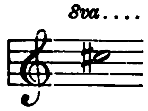

FIG. 2.

All this is true on the assumption that it is a flute; now it is well to keep in mind the four classes of American aboriginal wind-instruments that have all been called flutes and of which only one should be so called.

(*A*) The oboe class played perpendicularly with a vibrating reed;

(*B*) The flageolet class, played perpendicularly, with no reed but with a vent-hole which catches the breath on its outer edge and gives a whistling sound;

(*C*) The "key-whistle" class, played perpendicularly, with no reed, but where the outer edge of the truncated tube-end catches the breath and makes the sound, as when one whistles in a key;

(*D*) The flute class, played horizontally, with the breath projected into a lateral mouth-piece, the so-called *flauto traverso.*

It is as yet to be determined in which class the Pecos object belongs; in the hope of clearing this up the collections at Andover and at the Peabody Museum of Harvard University were examined and the courtesy of Miss Frances Morris and of Mr. E. H. Hawley at the Metropolitan Museum of Art, New York and the National Museum at Washington, respectively, opened cases and spread out material.

No exact counterpart of the instrument was found in these collections, but on discussion with Mr. N. C. Nelson of the American Museum of Natural History, New York, some forty or fifty similar instruments were somewhat unexpectedly shown me. These are all from Pueblo explorations, mostly in the Rio Grande Valley south of Santa Fe.

Dr. E. H. Hewett, Director of the School of Research, Santa Fe, also told me that he had collected a fair number on the Pajaritan Plateau.

Geographically and archaeologically, the type seems to be highly localized.

As to the instrument itself, we are no nearer a solution. Two specimens in the National Museum show some analogy; No. 15825, marked "Rio Grande do Sul," and No. 4346, from British Guiana; in each of these the upper end is occupied by a stopping that has been perforated and each carries three finger-holes. An instrument resembling the latter is in the Academy of Natural Sciences, Philadelphia.[1] These three were of course played perpendicularly, as flageolets.

In the Metropolitan Museum there is a true flute of wood, possibly Apache; the catalogue in referring to this says guardedly that side-blown flutes in America are very rare and that they suggest outside influences.[2] All this goes to support a presumption that the Pecos instrument is a flageolet.

On the contrary the stoppings and the diaphragm are larger, more important and further down the tube in flageolets than in flutes and it is strange that no trace of gum, resin, or bitumen was observed by Mr. Nelson nor myself during our close study of the American Museum specimens; this fact with the absence of such traces in our specimen leaves open a chance that it is after all a flute.[1]

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[1] Thomas Wilson, *Report, U. S. National Museum*, 1896, p. 650.

[2] Frances Morris, *Catalogue of the Brown Collection of Musical Instruments*, p. 127.

PHILLIPS ACADEMY,
ANDOVER, MASSACHUSETTS.

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[1] Mr. Oric Bates, Curator of African Ethnology in the Peabody Museum tells me that he observed a man of the South Mediterranean stock on the Siwa Oasis playing an exactly similar instrument as a flute.

EVIDENCE OF CIRCULAR KIVAS IN WESTERN UTAH RUINS

By NEIL M. JUDD

DURING the course of a recent archaeological reconnoissance[1] of western Utah, conducted under the auspices of the Bureau of American Ethnology, two distinct types of prehistoric habitations were observed: (1) Rude huts, circular in shape and built of leaning logs covered with layers of osiers, grass, and clay, in succession; and (2) rectangular houses the vertical walls of which had been constructed entirely of adobe mud. Habitations of this second type commonly occur in groups, forming villages or communities of detached dwellings the relationship of which, one to the other, suggests that coming together of individual families or clans which later resulted in the complex, many-storied homes of the ancient cliff-dwellers and the modern Pueblo Indians.

Two such groups have already been completely exposed, one at Beaver City, in Beaver county, the other at Paragonah in Iron county. In both of these aboriginal villages the principal dwellings were rectangular in outline; their walls had been constructed of superposed masses of plastic adobe, forced into position and smoothed by the hands of the workmen.[2] The roofs of these houses had been flat and built after the manner most common to primitive structures throughout the Southwest. Absence of lateral doorways indicates the prevalence of roof openings and the probable use of the roof as a platform whereon many of the lesser household tasks were oftentimes performed.

The present paper, however, has no interest in mere habitations, except in so far as they relate to adjacent buildings the remains of which bear certain similarities to circular kivas of the San Juan drainage. The wall fragments of one such structure were dis-

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[1] *Smithsonian Miscellaneous Collections*, vol. 66, no. 3, 1915; vol. 66, no. 17, 1916.

[2] See *Holmes Anniversary Volume*, p. 250.

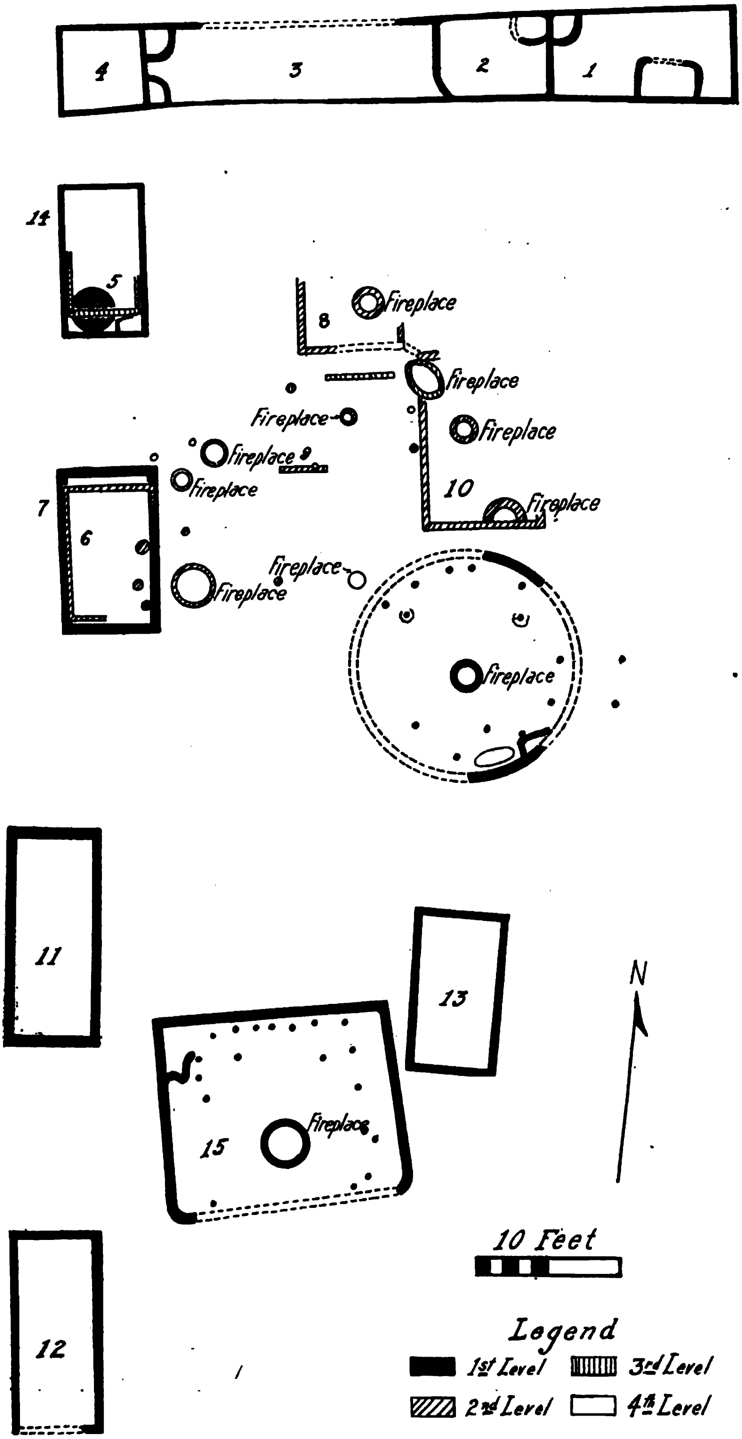

FIG. 3.—Ground plan of Beaver City, Utah.

covered in each of the two house-groups before mentioned; it is to these remains that attention should be directed for the presence in western Utah of circular kivas, if such they be, occurring in villages of detached adobe dwellings, must have a very obvious bearing upon our present conception of the origin and distribution of the ancient Pueblo peoples.

In the Beaver City mound the relative position of the circular room to the neighboring houses is indicated by a ground plan (fig. 3)[1] of the prehistoric village. It is well to note that although four distinct levels of occupancy were observed here, the upper three could not be traced in their entirety, and were most apparent only in that portion of the mound lying between the so-called kiva and the northern series of rooms. These superposed layers, however, seem to represent chiefly the rapid accumulation of campfire debris and wind-driven earth in and about temporary structures which sheltered most of the domestic activities of the little community.

Of the walls in the circular room only two small segments remained. These were of adobe, not more than eight inches thick, and had been constructed in the same manner as those in the secular structures. Occupying approximately the center of the building was a rimmed fireplace; about this were four postholes, marking the former position of roof supports. Smaller postholes, following the inner circumference of the room, suggest that additional pillars were found necessary after completion of the structure.

Attention should be directed to two similar holes just outside a continuation of the eastern wall. Although nothing else remained to justify the belief it seems not unlikely that thin walls, connecting the posts which filled these holes with other posts standing just within the room, may have formed a passageway which served approximately the same purpose as the so-called ventilator or ceremonial opening in circular rooms of the San Juan drainage.

An adobe partition joined the south wall-segment, forming a triangular bin. At the western end of this, occuping a shallow, basin-like depression and covered with a two-inch layer of tamped earth, lay the skeleton of a middle-aged male. The presence of the burial resulted in a low platform, the general appearance of which warranted the belief that it had proved no serious obstacle to the ceremonies which subsequently occurred within the building.

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[1] *Proceedings of the Nineteenth International Congress of Americanists*, p. 121.

The ruins of a second circular room, similar to the first, were exposed in the spring of 1916 during the course of excavations at

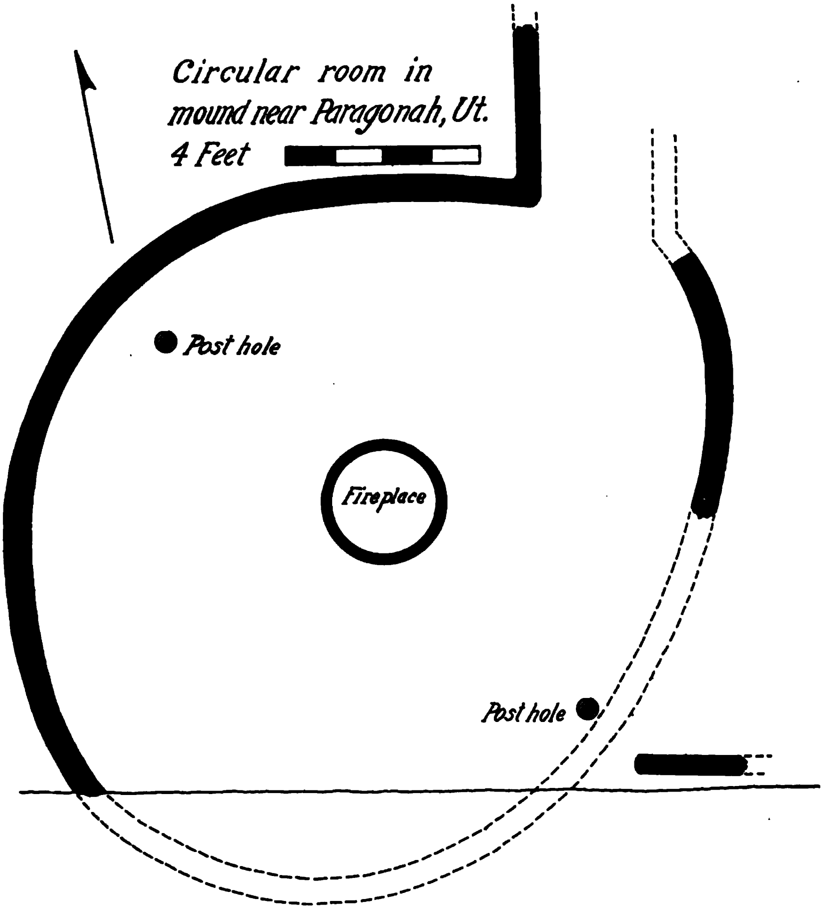

FIG. 4.

Paragonah, about forty miles south of Beaver City. In general, the buildings comprising this second house-group formed a less compact community than those in the village first mentioned.

Like it, however, the Paragonah mound furnished evidence that temporary structures were erected with comparative freedom between the rectangular dwellings and even above the ruins of razed houses.

Of the lower walls in the Paragonah kiva, nearly one-half remained in an upright position. The northern side, although only twenty inches in height, sloped outward twenty-two inches from the perpendicular and may account for the early abandonment of the structure. That repeated attempts had been made to check the settling of the wall at this point is evident both from the quantities of debris which had been banked against the outer side and from the several superposed coatings of adobe plaster, each one of which proved thicker at the top than at the bottom.

That this structure had been circular in form is plainly shown by the accompanying diagram (fig. 4). It will be noticed also, that an opening or passageway, one wall of which still remained, appears at the northeastern side of the room—an opening which may be likened to similarly orientated passageways occurring in recognized kivas. In the middle of the floor is to be found the usual rimmed fireplace; it was filled with sand and ashes, and covered by masses of burned roof adobe, each bearing impressions of the timbers, etc., upon which it had formerly rested.

Unlike that in the circular structure at Beaver City, the roof of this building had not been supported by posts set about the fireplace. For this reason it seems entirely plausible that the weight of the heavy covering had proved too great for the thin earthen walls, finally causing their collapse as noted on the northern side. Postholes on opposite sides of the room indicate that two pillars had been erected before final abandonment of the room.

A third circular building (fig. 5), plainly a kiva, was associated with a small group of unattached cliff-houses in Cottonwood canyon, near Kanab in Kane county, a few miles north of the Arizona border. The structure averaged fourteen feet in diameter; its highest standing walls measured six feet. A fireplace, lined with stone, occupied the middle of the floor.

Excepting at the eastern quarter, the lower wall of this room was faced with sandstone slabs, averaging twenty inches in height. One of these covered the opening to a small tunnel the outer end of which had been destroyed during accumulation of the talus in front of the cave. The entrance to this passage had been constructed with considerable care. Cedar sticks formed a lintel upon which rested the superposed masonry; the door already mentioned, a stone slab slightly smaller and more carefully trimmed than the others, leaned against upright cushions of adobe, standing within and at each side of the opening. From the quantities of roofing material extant it is evident that the room had been covered with successive layers of logs, willows, cedar bark, and clay, and that the roof had remained in position long after abandonment of the village.

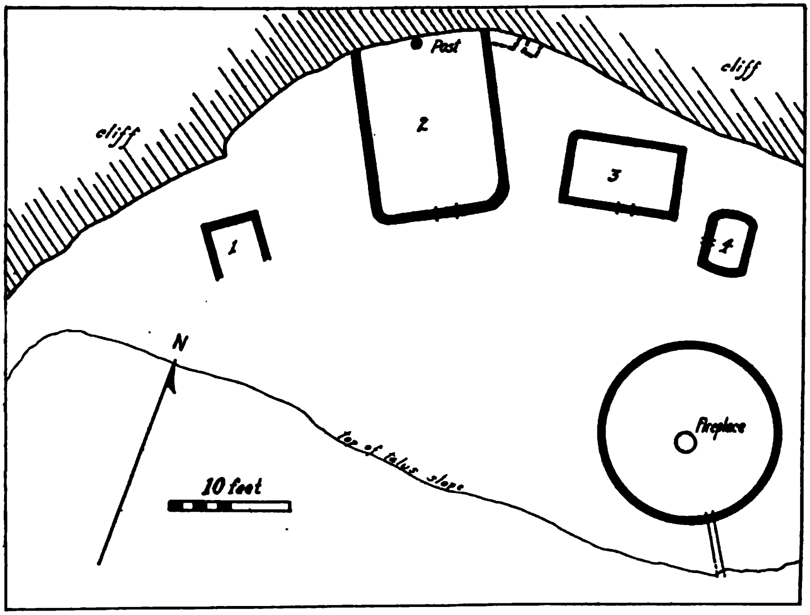

Fig. 5.—Plan of structures in Cottonwood canyon, Arizona.

This kiva, like the circular, adobe rooms in the Beaver City and Paragonah mounds, was a plain-walled structure, being furnished with neither buttresses, banquettes, nor wall pockets for the storage of ceremonial materials. As in the others, also, a round fireplace occupied the approximate center of the building but no indications occur of other recognized kiva furniture.

From an examination of their meager remains and without especial consideration of the probable relationship existing between them and the adjacent secular rooms, it would appear that the circular structures associated with villages of ancient adobe dwellings throughout western Utah are in fact ceremonial chambers and that, while not wholly like the well-known kivas of the San Juan drainage, they are, nevertheless, structures which may have exerted a very marked influence upon the round rooms in the locality last mentioned.

The archaeological reconnoissance of western Utah is not yet complete but the results so far obtained indicate that at least one of the units which comprised the complex cliff-dweller or prehistoric Pueblo culture found its origin north and west of the Rio Colorado. It seems evident, furthermore, that there originated in the semi-desert valleys of western Utah a circular type of ceremonial room which, if not the immediate precursor of the kiva as found throughout the entire San Juan drainage, at least played a very important part in determining the final form of that structure, now so generally recognized as the focus of most communal activities in prehistoric Pueblo villages.

United States National Museum,
Washington, D. C.

SIMILARITIES IN CULTURE

By W. D. WALLIS

THE concept of convergence is generally accounted a twentieth century achievement and a departure from Darwinism. Yet Darwin recognized the possibility of convergence and, to a certain extent, even defended it. He writes in the *Origin of Species*,

> I am inclined to believe that in nearly the same way as two men have sometimes hit independently upon the same invention, so natural selection, working for the good of each being and taking advantage of analogous variations, has sometimes modified in very nearly the same manner two parts in two organic beings, which beings owe but little of their structure in common to inheritance from the same ancestor.

In 1878 Professor Flower applied the concept to the problems of physical anthropology. The general appearance of the Australians,

> all their cranial and skeletal characters ally them so closely to the Melanesians, and also to the African negroes, that it is extremely difficult to suppose that so many coincidences could have arisen in two stocks which had already diverged so far as to fix permanently the distinctive characteristics of the hair. Again, take the Negritos of the Malay Archipelago. Here we have a wooly-haired people, with scarcely any of the osteological and perhaps cerebral characteristics of the other negroid races. The alternative supposition that wooly-hair could have originated independently, upon different branches of straight-haired races, is also beset with difficulties. It is clear, however, that setting aside the doctrine of separate creation, one or other of these events must have taken place; but which is the more likely is impossible, in our present state of knowledge, to decide.[1]

Similar difficulties led the British zoölogist, R. L. Sclater,

> to question whether identity of structure must be taken, *without exception*, as an indication of immediate descent from a common parentage.[2]

The anthropologist, in his belated way, accepts the results and methods of other sciences and attempts to apply them to his own problems. The problem of convergence was treated at some length by Otis T. Mason in 1895,[1] and briefly by McGee, a few years later.[2] Frequent reference is made to convergence in contemporary works,[3] and in the past decade several articles have been devoted to this subject.

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[1] *Proceedings Royal Institute*, vol. VIII, 1878.

[2] *Ib.*

For Mason the problem resolves itself into fortuitous coincidence versus causal interconnection and a calculus of chance must be employed in the absence of historical evidence. Goldenweiser[4] adds to this a further qualification: We must note the possibilities of development and this will aid us in determining whether the similarity is due to necessary choice from among a few possible ones. If such limitations exist, then similarities are inevitable whether there is historical contact between the two cultures or not. Thus convergence is assured, provided we can discover the limiting conditions plus the inevitable choice. This is what we hope to make clear by the application of this principle to some concrete situations.

LANGUAGE

That there are numerous similarities between some Hebrew syllables and some syllables pronounced by some American tribes is not surprising in view of the limited means of vocal utterance and the large number of combinations employed in each language. The greater the number of languages chosen for comparison, the greater the probability that correspondences will be found among them, since the means of vocal utterance are not unlimited.

MATERIAL CULTURE

The same inevitable correspondence may be anticipated if, instead of language, we take the trivial or external generally, such as paddle-blades, pottery, baskets, stone implements. A stone implement is either natural or but crudely shaped (eolithic), chipped (paleolithic), polished (neolithic), or pecked. The possibilities of fashioning may be limited by the kind of material employed. This large class of similarities we may call the trivial or external because they result from the limitations in the environment rather than from conditions inherent in social and psychic life.

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[1] "Similarities in Culture," *American Anthropologist*, vol. 8, 1896.

[2] "The Trend of Human Progress," *American Anthropologist* (N. S.) vol. 1, 1889.

[3] See, for example, E. Westermarck, *Marriage Ceremonies in Morocco*, p. 7, 9; E. S. Hartland, *Ritual and Belief*, p. 268.

[4] "Principle of Limited Possibilities," *Journal of American Folk-Lore*, 1913, p. 278.

PHASES OF THE SOCIAL LIFE

When we examine the life of various peoples, whether primitive or advanced, we find threads of similarity running throughout the complexity of the various cultures which, at first sight, seem amazing. In such remote tribes as those of Australia, of Africa, of North America, there are certain common modes of social and psychic life which exhibit strikingly similar motives however variously expressed. In all of them are sharply drawn lines of sex which give to the male and female different social privileges and duties, different economic pursuits, different dress and adornment, different religious rites, often a different theology, sometimes even a different code of language. In all of them, as also in the higher cultures, the age of the individual is an index to social position and influence; so are burial ceremonies, which everywhere reflect the social position and influence of the deceased. There is everywhere some regulation of sex relations, a recognized form of marriage, incest prohibitions and compensating privileges. There is always an individual or a group of individuals in whom if not continuous at least temporary and dominant social authority is resident.

That culture should express itself in these fairly definite if elastic moulds, each with its peculiar content, is, perhaps, not surprising in view of the conditions of culture. Culture is inseparable from social organization. Its development demands at least two things: first the cooperation of many individuals, and second, a continuity of effort, of accretion, and of readjustment that only a larger span than the life of an individual makes possible. This continuity of development is a vain hope unless guarded and guided by a self-protecting organization in which its own life is incorporated. If unification of individual purposes becomes necessary when danger threatens the group life, no less necessary is some center of authority represented either by an individual or a group of individuals in whom the social purpose can become articulate, so that it can receive definite expression readily intelligible to all. In this way common intention can focus with the advantage to the group of its own rebound. The social life must somewhere become articulate in order to appreciate its own purposes and profit from its own counsels. Thanks to this conditional and temporary surrender of authority to a central source the entire social life can be coordinated, and the force of its concentrated effort brought to bear on the solution of its problem, be this a danger arising from within or one imposed from without. A central coordinating authority is as necessary for society as for physiological structure.

The analogy is further applicable. In certain amorphous societies, such as the Eskimo, we find, as in the amoeba, little apparent coordination from a single center. As the organism becomes more complex there is a correspondingly greater demand for a center of coordination. In the more complex African cultures this center of authority is much more insistent and much more highly developed than in the cruder cultures of that continent or of Australia. In the islands of the South Sea generally, with more complex culture and greater social differentiation we find increasing importance attaching to chieftainship and to its perpetuation in the direct line of descent.

As we go from the lower cultures to the higher, that is to say, from the simpler to the more complex, we find increasing tendency on the part of the central authority to exist for its own sake and apart from the needs of the tribe or nation. In the cruder societies the authority is always subservient to the greater social needs and develops as a feature subsidiary to tribal demands. For example, in the simpler societies, especially in those that are continually facing danger, we find a more ready and consistent recognition of personal merit as a qualification for leadership than in the more advanced societies. The small and unfederated tribes of North America and of Australia, demand fitness for office from their chiefs and leaders. This is less often the case in the more complex cultures of Polynesia, as also in the more highly evolved African cultures.

In European societies the highest offices have frequently, in time of peace, been filled by standards implying no stress upon personal fitness. Here the culture feels able to take care of itself. Danger from an outside foe is not immediately apprehended as it is in the small group whose every member must be in the fore of the firing line. The individual assertion, the initiative which carries the leader to his position of influence and authority at the head of the tribe, as we find more frequently occurring than not in the lower societies, is the fulfillment of a social demand enforced by the harsh conditions of menace from without, ever threatening the integrity of the tribal life.

MAGIC

The wide distribution of the types of contagious and homeopathic magic is not a mystery. The law of the association of ideas is no respecter of clime, culture, or previous education. The two ideas meet and leave their implication. Even we civilized people are sensitive about our name or photograph or personal appearance, remarks about which we take to heart to a degree less absurd than the savage way but scarcely different in kind.

MESSIANIC RELIGIONS

If, again, we ask why messianic religions should be found only in dissatisfied tribes fretting under outside compulsions, or among the oppressed classes, we may find an answer in the fact that for a satisfied people there is no demand for a change because no motive for it. Genuine dissatisfaction tends to develop into ardently hoping and actively looking for some way out of the difficulty. People grasp at straws only when in dire straits, but in such situations they will grasp even at straws. In such circumstances credulity, as exhibited in these desperate attempts at self-preservation, rises with the danger; any hopeful testimony will be heard and often will be followed if only it promise salvation.

BURIAL RITES

Without an appreciation of the extent to which many similarities in culture are not accidental but incidental to it we are likely to miss their meaning. Dr. Rivers' observation that the burial customs of a people change slowly may be true enough, but there should be coupled with it some statement of the meaning that the burial rites have for the people who practise them. In Australia, and in most tribes, the social personality of the deceased does not die with the body but passes beyond the death portal. To the body is shown about the same degree of respect that was shown the deceased while alive. The bodies of women are seldom disposed of like those of men, nor those of children like those of adults. The bodies of chiefs and braves are interred in different manner from those of the common people, and even in the same tribe and family a favorite child may be disposed of in one way and that of one who is not a favorite in different manner. The body of a female child is not disposed of as is that of a male; old women are not buried as are young ones. The different methods of disposing of the dead of which Dr. Rivers speaks, may be found in a single tribe. There is no need to call in accidental outside influences when demands and standards incidental to the culture suffice to explain these differences; they do not, of course, explain why *this* rather than *that* form became prevalent in a particular tribe.

ESCHATOLOGY

The belief in the survival of the soul is probably universal. Being so general it might seem to be sufficiently accounted for by borrowing since there are no gaps in the geographical distribution. Yet even if its distribution is due to borrowing, we are left the question why this belief rather than some other, should so easily and so tenaciously attach itself to every culture. The problem of its universality remains much the same whatever the story of its development.

The animistic theory explains nothing—it merely describes. William Hazlitt has given us a more fruitful suggestion:

> I have no wish to have been alive a hundred years ago, or in the reign of Queen Ann: why should I regret and lay it so much to heart that I cannot be alive a hundred years hence in the reign of I cannot tell whom?
>
> When Bickerstaff wrote his Essays I knew nothing of the subjects of them; nay, much later, and but the other day, as it were, in the beginning of the reign

> of George III, when Goldsmith, Johnson, Burke, used to meet at the Globe, when Garrick was in his glory, and Reynolds was over head and ears with his portraits, and Stearne brought out the volumes of Tristram Shandy year by year, it was without consulting me: I had not the slightest intimation of what was going on: the debates in the House of Commons on the American war, or the firing at Bunker Hill, disturbed not me: yet I thought this no evil—I neither ate, drank, nor was merry, yet I did not complain: I had not then looked out upon this breathing world, yet I was well, and the world did quite as well without me as I did without it! Why, then, should I make all this outcry about parting with it, and being no worse off than I was before? There is nothing in the recollection that at a certain time we were not come into the world that "the gorge rises at"—why should we revolt at the idea that we must one day go out of it?

The fact is, we do revolt.

Why is the belief in post-mortem existence so much more general than the belief in pre-natal existence? Why look to the future rather than to the past, lengthening the short span of our life in only one direction rather than extending it both ways at once? Why the sentimental, highly emotional, and dogmatic attitude toward the belief in post-mortem existence, whereas the belief in pre-natal existence can be discussed on the basis of its own merits?

The human mind, because of conditions essential to its existence and beyond its control, is predisposed to believe in a future life. In fact, the belief is an almost inevitable outcome of naïve psychology. Practically the whole set of our life is to the fore. We are purposive beings actuated by intentions and ideals whose fulfillment is necessarily postponed to the future. As the struggle waxes, and the issues become more vital, increasing attention must be paid to what lies before. To rivet our attention upon the past, whether in admiration of its achievements or in regret over our absence from that phase of historical development, is to risk a future benefit which may be snatched from us by another. We must achieve our fortune where the fates have cast our lot. To prefer the future, to look forward to it as the realization of long-continued efforts is essential to the progress of the individual who, otherwise, has no rewarding motive, no effectual stimulus.

To wish ourselves back into a past is, on the face of it, a hopeless wish. To wish ourselves participants in a future situation is not necessarily absurd. It is a principle that we apply in every hour of our waking consciousness. Moreover it is productive of results: it brings things to pass; it gets us where we wish to be. Absence of it is a handicap in the struggle for existence. The will to believe becomes irresistible. Besides, to be excluded before the game of life begins, is not so harsh as to be ruled out while hopefully participating. The conventional game has its completion but the game of life, never. There seem always moves ahead of us; the situation is always changing; in fact, it changes with such rapidity that only the most agile can continually and successfully readapt himself to every important phase of it. The participant in the game of life sees no definite completion of it, no last move beyond which is nothing. He never has come to an *impasse* and his hope will hurdle death itself.

MYTHOLOGY

A few years ago the writer advised

> a caution against too rapidly inferring contact from a study of correspondence in folk tales. No one who has familiarized himself with the Hopi tales can fail to detect the likeness between the role of Leopard in the Mpongwe tales and of Coyote in Indian mythology. Even in such details as Coyote grasping the tail of Rabbit and letting go on being told that he has not Rabbit's tail but some other object we find parallels. The blowing of pepper by Rat into Frog's eyes is similar to the blowing of the flame and hot tar into Coyote's eyes, and Leopard lying prostrate feigning death, having sent his wife to proclaim his death and entice Rat within the danger line, is like Coyote's same trick when conspiring with Skunk to get prairie-dogs and rabbits within his reach. In a word, if one were to judge from the evidence without the hint it carries of respective location, the conclusion that there had been transmission from one area to the other would be almost irresistible. Yet this shows that we may have in contiguous, or almost contiguous areas the same type of stories in many marked particularities seeming to point to a common origin, while they may really have had quite independent origins.[1]

Myth-making is so universal that if the similarity in two myths of widely separated tribes is such as to imply borrowing, it is because of the number of similar accidental traits. If the similar features are such as may be accounted inherent in the respective cultures, the trickster motive, for example, then, lacking historical evidence, independent origin is as good an explanation as borrowing.

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[1] *Current Anthropological Literature*, I (1912), pp. 207–8.

CUMULATIVE EVIDENCE

Again, we have puzzling parallels between the Hopi and the Thonga. Not only do we find many incidents of the Hopi tales occurring among the Thonga, but in the beliefs with regard to the moon and the morning star we find notable similarities. Thus, as in many parts of America, the Hopi believe that when the horns of the crescent of the moon are turned up dry weather is foreboded, when turned down, rain. This, of course, may be European, as may also the Thonga belief that when the horns were turned down,

> this showed there was nothing to fear, all the dangers contained in this month had been poured out: "the assagais were dispersed." On the contrary, if the horns were turned toward the heavens, this showed that the moon was full of weapons and misfortunes.

The importance which the Hopi attach to the morning star has its analogue among the Thonga who greatly laud it as the herald of the day. To it the Hopi warriors offer prayer for success in the coming fight. Among the Thonga,

> it gives the warriors the signal for starting on a war-like expedition: the warriors can easily kill their enemies under cover of the darkness, and the sun will soon appear and help them to complete their victory. Lighted with its light, candidates of the circumcision leave the village of the chief to go to the house of initiation,

—again similar to Hopi ceremonial.[1] A further similarity between the two cultures is to be found in the creation myths which state that the ancestors emerged from a world below the surface of the earth, the Hopi progenitors climbing up on the limbs of the piñon tree, while, among the Thonga, Khudjana planted a big pole in the earth, and passed spokes through it, right to the top. Ribiubi having climbed up, Khudjana took out the lower spokes and went home. His father as a result was not able to get down. Among the Hopi, some were not allowed out because of their evil deeds, a cover being placed over the place of exit to prevent their entrance into this world. Among the Hopi, a nephew of the first chief upon earth announced that those who died would live again in the spirit world and pointed out the spirit of a man whom he had killed. In both areas a chief was found to be already upon the earth when the underworld people arrived. The Thonga say that

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[1] For Thonga parallels see H. A. Junod, *The Life of a South African Tribe*, II, pp. 283, 286.

> the chief of this marsh sent the Chamelion to them with this message: "Men will die but they will rise again—"

—a familiar strain in American creation myths.

Another parallel is to be found in the superstition about counting the stars. I am not aware that this superstition has been recorded among the Hopi but it is found among the neighboring Siouan tribes with whom the Hopi have much in common. The Sioux believe that if one starts to count the stars, even mentally, misfortune will overtake him unless the count is completed. The Thonga say, similarly, that "counting the stars represents the torment of the soul!" If a child has been deprived of food as a punishment for an offence, his parents will tell him, when he goes to sleep: "Go and count the stars," meaning, "you will be hungry and not be able to get to sleep; you will be as unhappy as if you had to count the stars."

SIMILARITY IN ACCIDENTAL AND DETACHABLE FEATURES

So far as the resemblances rest in non-essential and detachable features they imply borrowing. The value of this principle is pointed out by Sapir in his critique of Von Hornbostel's attempt to show cultural contact between South America and Melanesia.

> The absolute pitch of a tone is, musically speaking, an irrevelant matter, the essential thing being always the intervalic relations between the tones. Thus, while intervals and scales are "constitutive" or technically essential factors, absolute pitches are "accessory" and fulfill the second requirement of "absence of purpose." Two scales that are similar need not for that reason be historically connected. If, however, to similarity or identity of scale is added practical identity of pitch of the homologous tones of the scales, it becomes impossible or, at least, exceedingly difficult to believe that they are independent in origin. And if, lastly, parallel scales of practically identical pitches are found associated with musical instruments of nearly identical construction, the certainty of historical connection is certainly beyond reasonable doubt.[1]

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[1] *Current Anthropological Literature*, II, pp. 69–72.

This is clearly stating the criterion to be applied, namely, that of accumulating accidental features.

THE LAW OF PROBABILITY

G. Staniland Wake once remarked that the "possible in social life may reasonably be expected to occur somewhere or other on the earth's surface," an observation supported by the element of freedom in social life and the varied opportunities for expression. The possible occurrence of a given phenomenon is not exhausted when it has appeared somewhere once. When the men of Barca, according to Herodotus, made a treaty with the Persians, "So long as the earth whereon we stand shall abide, so long shall our covenant endure," this did not preclude the Indians of America then, or centuries later, making treaties under the form, "So long as the sun and moon shall shine," or, "So long as the tides shall flow and the grass grow." But is it possible to construct a law of probability with regard to the independent development of similar features? If any principles of social development can be adduced, such a law is certainly possible. When we attempt to interpret a given example by means of this law we will, of course, often go astray, if we anticipate surety. The insurance company, also, frequently goes astray when insuring a given individual, but its financial success is standing proof of the correctness of its general assumption. The fact that the law itself is only tentative, subject to constant revision as the conditions under which it operates become clearer, does not invalidate the attempt to formulate such a law. At first glance, for example, the similar conditions prevailing in those tribes where messianic religions have flourished, seem to supply all the conditions necessary for independent origins; yet its distribution turns out to be one of geography almost as much as one of psychology, though it is never purely a question of geography.

SUMMARY

The importance which we are to attach to similarities, in the absence of historical information, must be determined inductively from two supplementary points of view: First, to what extent the features are accidental and indispensable, or incidental and inherent; second, the number of such similarities in the cultures. No conclusion with regard to independent origin or borrowing, will, in the end, where there is absence of historical proof, be more than tentative. I cannot agree with a recent writer who insists that we must be able to trace a distinct line of historical transmission before we are entitled to say that the ideas of one people are derived from those of another, or influenced by them.[1] The correspondence may be of such a kind that independent origin seems precluded and historical continuity must be assumed. But it is, after all, only assumed. I find it safer to agree with Sapir[2] that the similarity may be such as to make borrowing very probable, but, as he seems by implication to agree, cannot rule out the possibility of independent origin. Given ten thousand isolated tribes, each developing a musical scale, might we not expect, as between some two of these, a fairly thoroughgoing resemblance in tones, in pitch, and in scale? Moreover, when the instruments—which may have been evolved independently out of similar environments, as, for example, when bamboo is employed—are similar, the probability of independent origin is heightened.

In many cases instances of convergent evolution turn out to be, when viewed from a wider perspective, instances of genuine divergent evolution. Darwin and Wallace are often cited as examples of two men hitting upon the same idea (natural selection) almost simultaneously and independently. So far as we confine our attention to these two individuals, the similarity is one of convergent evolution. But if we take a more comprehensive point of view, with an eye single to the culture, then the two theories of the two men are but two expressions of the logical outcome of the thought and observations of their nation and age. Malthus had done all except express the principle clearly; the thing was in the air, and someone was bound sooner or later to give expression to this logical outcome of prevalent thought. Darwin and Wallace were the vehicles of this train of thought, which had progressed thus far, and each gave it its logical outcome. Thus, from the point of view of the culture, we have divergent evolution; from the point of view of the individual, convergent evolution.

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[1] S. D. F. Salmond, *The Christian Doctrine of Immortality*, Edinburgh, 1903, p. 21.

[2] *Op. cit.*

Another instance of what is both convergent and divergent evolution,—convergent if we take the individuals as the basis of consideration, divergent if we take culture, in this case western civilization, may be found in the preparedness movement or movements, initiated in the United States after the beginning of the European war. There are, of course, more ample examples of borrowing of the idea, but there is no doubt that, to many individuals, because of similar motives and the international situation, the idea came quite independently of any neighbor's suggestion. But if we are comparing culture areas, then these are so many instances of a divergent evolution which would have required no very astute student of political life to anticipate. So, too, in primitive life, the similarity in two or more cultures is independent origin and convergent evolution if the tribes are the units of comparison, but often a true divergent evolution if more comprehensive culture backgrounds are chosen as the units of comparison.

That instances of independent origin can be found, even when very comprehensive culture backgrounds are chosen for comparison, can be shown with high probability, though never, perhaps, with absolute demonstration. It is not probable that the concentric circle of Australia has been derived from or given rise to the concentric circle in western Europe of the Bronze Age period, and there is no probability of a common origin. It is not probable that the sewed and pitched bark canoe of the northern part of northern territory, Australia, has any historical relation with the sewed and pitched bark canoes found among the Northeastern and Central Woodland Algonkian. Even in details of social and psychic life we have striking correspondence where there is no probability that a common source gave rise to the two beliefs. Two examples will suffice. Almost concurrently there arose in the Plains area of North America and in the Egyptian Soudan, messianic movements —the Ghost Dance religion and Mahdiism. Both promised much the same thing to a people who were suffering from practically the same kind of oppression. The Soudanese Mahdi assured his soldiers that the bullets of the foe would have no power to kill but would fall harmlessly from their bodies to the ground. The North American prophet made the same promise, and in both cases they were believed. Again, in the fifteenth century, a Hebrew messiah foretold an eclipse of the sun. He was not, at the time, given credence, but fulfillment of the prophecy was accepted as proof of his messiahship. There was a thoroughgoing parallel in the Plains area during the last century. As the Hebrew instance was known to western Europe only some decades after the occurrence of the North American episode, there is no probability of any mutual influence even through an indirect channel. Examples can be multiplied; but one should be as good as a thousand.

The problem of convergence is, in fact, as old as Buddhism and the solution there posited is as modern as any of our own day. Here it is in a nutshell:

> A man throws a perforated yoke into the sea. The winds blow it in different directions. In the same sea there is a blind tortoise, which after the lapse of a hundred, a thousand, or a hundred thousand years, rises to the surface of the water. Will the time ever come when that tortoise will so rise up that its neck shall enter the hole of the yoke?

The Buddhist answer is,

> It may; but the time that would be required for the happening of this chance cannot be foretold.

It cannot be foretold. The time required varies as the size of the sea, of the yoke, and of the tortoise—not to mention a thousand other factors that enter into the problem. But the unexpected must be expected to happen sometimes. Out of the many cultures known to us, remarkable and nearly thoroughgoing similarities may be anticipated even where the course of development has been an autochthonous growth and not a grafting. For in social life there is an element of freedom and the possibilities of expression are almost infinite.

FRESNO, CALIFORNIA.

DE SOTO'S ROUTE FROM COFITACHEQUI, IN GEORGIA, TO COSA, IN ALABAMA[1]

By DANIEL MARSHALL ANDREWS

THIS investigation embraces that part of De Soto's wanderings, from the town of Cofitachequi to Cosa. The sites of these two places have been fairly well established: Cofitachequi on the east, at Silver Bluff, on the Savannah river, twenty-five miles by river below Augusta, Georgia; Cosa on the west, near the mouth of Talladega creek on the Coosa river, about two miles above Childersburg, Talladega county, Alabama. An effort has been made to locate the route traveled and the various town sites mentioned in the De Soto narratives.

The writer believes he has approximately located Xuala; that he has definitely located Gausili, Chiaha, Coste, Tali, Connasauga and Tasqui. He has brought to his aid in the investigation the geological and topographical knowledge of the country traversed, and his own intimate, personal familiarity with the region.

He began this study believing that the site of Chiaha was on the Tennessee river, because Professor Halbert had so located it, and he laid great store by Professor Halbert's opinion. It was only by the severest process of elimination that he finally abandoned this location, and it can not, therefore, be presumed that he began the investigation with any bias in favor of the Coosa river.

That part of De Soto's route with which we are concerned can be generalized as follows: From Cofitachequi to Chalaque (in the latter we recognize the modern word Cherokee) he marched through the coastal plain; from Chalaque to Xuala, through the Piedmont plateau; from Xuala to Gausili, through the Appalachian mountains; and from Gausili to Cosa, through or rather, down, the great Paleozoic valley, lying west of the great Cartersville fault. The descriptions of all the chroniclers confirm this generalization; and thus the scope of our investigation is narrowed to a study of this geological region in Georgia and Alabama. Referring to the geological map (fig. 6), of that part of the state of Georgia with which we are concerned, it will be seen that the northern boundary of the coastal plain extends from Columbus on the west, through Macon and Milledgeville, to Augusta on the east. The Piedmont plateau extends from the northern limit of the coastal plain to a line drawn, roughly, northeast and southwest through Franklin county, approximately parallel with the Cartersville fault. The Appalachian mountains lie, approximately, between this line and the Cartersville fault, which is a great horizontal earth movement, extending, roughly, from the corner of Georgia and the Carolinas to Esom hill, in Alabama; and ten to twenty miles west of the fault lies the great Paleozoic valley.

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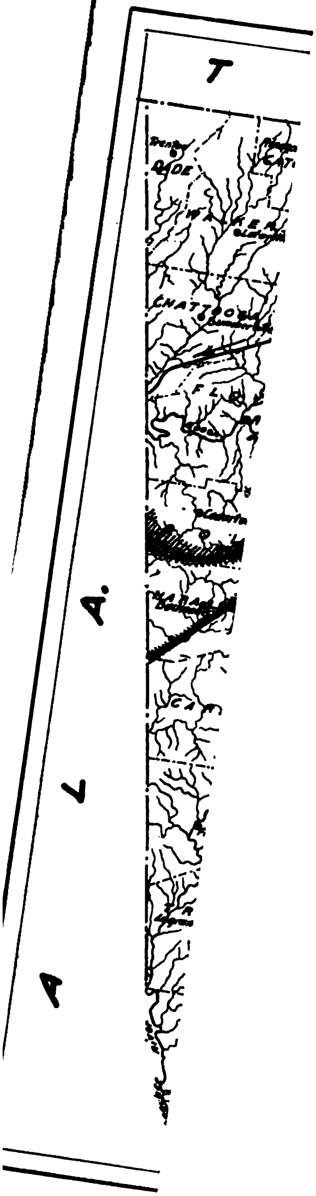
[1] Presented at a meeting of the Alabama Anthropological Society, held in Montgomery, Alabama, July 24, 1916.

Again referring to the geological map: there are indicated thereon the coastal plain, the great metamorphic or crystalline region comprising the Piedmont plateau and the mountain section, and the great Paleozoic valley. Within these limits lay the route of De Soto, in Georgia.

De Soto's route, as given by the chroniclers, is represented in the accompanying table. Ranjel is the only safe guide for distances traveled in days, because he is the only one of the chroniclers who always gives the actual number of days of travel and the days lost at each town, village or camp. The others frequently give distances in days that include stops at towns of which no mention is made; therefore, in computing the rate of travel in miles per day, Ranjel's narrative has been used exclusively. Ranjel's account of the expedition has been in print only during the last ten or fifteen years; the older investigators, following the narratives of the other chroniclers, have been misled into reading too great distances into the narratives. Ranjel's spelling of place-names has also been followed throughout, except in quotations. La Vega's account of the expedition is not used, except where his statements are obviously correct, or where corroborated by one or more of the chroniclers.

As will be shown further on, forced marches were made, because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of the scarcity of food, from Cofitachequi to Chalaque, from Chalaque to Xuala, and from Xuala to Gausili. An inspection of the table of distances shows that eighteen miles was a day's travel over this part of the route, except between Graves mountain and Xuala, where the travel was fifteen miles per day. From Gausili to Connasauga food was plentiful, and a day's travel was ten miles. From Connasauga to Chiaha part of the way was mountainous—"through a desert"—therefore more than twelve miles a day were traveled. From Chiaha to Cosa the journey was through a populous country, where food was plentiful, and a day's travel was as follows: From Chiaha to Coste, eight miles; from Coste to Tali, three miles; from Tali to Tasqui, seven miles; from Tasqui to Cosa, nine miles; and, though the rate of travel may not be absolute, it is relative, and the writer considers it strong corroborative evidence of his thesis.

TABLE

Ranjel					Biedma	Elvas	La Vega
Name of Town	*Location*	*Distance*			*Name of Town*	*Name of Town*	*Name of Town*
		Days	*Miles*	*Miles per Day*			
Cofitachequi	Silver Bluff				Cotifachiqui	Cutifachiqui	Cofaciqui
Chalaque	Near Augusta	2	36	18		Chalaque	Chalaques
At mountain	Graves Mt.	1	18	18			
Xuala	N. and S. fork Broad river	4	60	15	Xuala	Xualla	Chovala
Gausili	Carters, on Coosawattee	5	90	18	Gausili	Gausule	Gauchoula
Connasauga	Near junction Coosawattee	1	10	10		Canasauga	
Chiaha	McCoy's island	4	50	12½	Chiaha	Chiaha	Iciaha
Coste	Woods island	5	40	8	Costehe	Coste	Acosta
Tali	Mouth of Tallasahatchee	1	3	3		Tali	
Tasqui	Near mouth of Choccolocco	3	20	7			
Cosa	Near mouth of Talladega Cr.	2	18	9	Cosa	Cosa	Cosa

On Wednesday, May 13, 1540, the expedition left Cofitachequi. Ranjel says: ". . . and in two days came to the territory of Chalaque . . . and they bivouacked in a pine wood. . . ."[1]

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[1] *Narratives of De Soto* (Trailmakers' Series), vol. 2, 1904, p. 102.

The great pine forest that then covered the coastal plain extended fifteen to twenty miles north of the fault line at Augusta, along the Piedmont escarpment, and it was probably in this forest, near Augusta, that the camp was located. On Monday, May 17, the expedition left Chalaque and, according to Ranjel, "spent the night at a mountain."[1] This camp we can locate definitely at Graves mountain, in Lincoln county, because it is the only mountain in that whole section, and is a prominent feature of the landscape for miles around. The older investigators, as has been shown, did not have Ranjel's narrative to guide them and, therefore, missed this well-defined landmark.

To have made the distance from Cofitachequi to Graves mountain in three days, the Spaniards were forced to march between seventeen and eighteen miles a day. We learn, from the narratives, that food along this part of the route was scarce, and they were compelled to make forced marches between points of supply, which were Cofitachequi, Chalaque, Xuala, and Causili. Then, too, they were not encumbered with a large drove of hogs, as many had been killed and eaten before their arrival at Cofitachequi, to save the expedition from starvation.[2]

After four days' march from Graves mountain they reached Xuala. La Vega says that the expedition marched four to five leagues each day.[3] The Spanish league being 2.63 of our statute miles, they therefore marched each day ten and a half to thirteen statute miles. La Vega's statement is probably near the truth, except as to forced marches. On account of the scarcity of food,[4] they undoubtedly marched rapidly to Xuala, where food was plentiful.[5] Four days' travel from Graves mountain to Xuala, at fifteen miles per day, would have brought them either to the junction of the north and south forks of the Broad river, in Georgia, or to the junction of the Broad and Hudson rivers, both locations being near together. Ranjel says Xuala "is a village in a plain between two

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[1] *Ibid.*, p. 103.

[2] *Ibid.*, p. 95, *et seq.*

[3] *De Soto and Florida*, 1881, p. 374.

[4] *Narratives of De Soto*, vol. 2, p. 15.

[5] *Ibid.*, p. 103.

rivers."[1] La Vega says: "Chovala . . . situated between a town and a very rapid little river."[2]

Tuesday, May 25, they left Xuala and went over a high range, and the next day they camped in a plain where they suffered from the cold, although it was the 26th of May.[3] Elvas says: "From Cutifa to Xualla (is) mountainous country (more correctly, from Chalaque to Xualla); thence to Gauxule the way is over very rough and lofty ridges."[4] From Chalaque to Xuala the route was through the Piedmont plateau. The hilly, broken topography of this region Elvas calls mountainous; and, compared with the flat coastal plain through which they had been marching, it could well have appeared mountainous to him, particularly that section in the vicinity of Graves mountain over which they had passed. "The very rough and lofty ridges" from Xuala to Gausili are the Applachian mountains.

La Vega says: "As soon as they left Chovala (Xuala) they struck straight for the coast and turned in the form of a curve, to arrive at the port of Achussi."[5] An inspection of the route, as laid down on the map of Georgia and Alabama, (fig. 7), will show that this statement by La Vega is correct.

The march of five days could have brought them to any point on the arc of a circle shown on the map of Georgia, (fig. 7), drawn with Xuala as a center. Had they reached the northern point, indicated by a cross, on the headwaters of the Connasauga river, they would still have been in the mountains; though, in marching towards the Tennessee river, they would have crossed the Connasauga, which crossing would have tallied with the description of the chronicles. But the insuperable objection to any part of the route being along the Tennessee is that Cosa could not have been reached from Tali in five days, and it would have been necessary to cross Sand mountain; while the accounts of all the chroniclers show plainly that Chiaha, Coste, Tali, and Cosa were all on the same stream, and no mention is made of a mountainous or broken country.

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[1] *Ibid.*, p. 103.

[2] *De Soto and Florida*, 1881, p. 367.

[3] *Narratives of De Soto*, p. 104.

[4] *Ibid.*, vol. 1, p. 71.

[5] *De Soto and Florida*, p. 375.

An inspection of the map shows that the arc of the circle drawn with Xuala as a center cuts the Etowah river, near Cartersville, Georgia. The location of Gausili on this stream must be rejected, because its location there and the subsequent route to Chiaha does not fit the description of the chroniclers at all. Had the route down the Etowah been followed, the site of Chiaha must have been at the present town of Rome, Georgia, and to have reached it by the route described by the chroniclers, the crossing at Connasauga must have been on the Etowah; the route, then, led over the ridge separating the valleys of the Etowah and Oostanaula rivers, thence down the Oostanaula to Chiaha; but the expedition would have entered Chiaha without crossing any stream; whereas, the chroniclers mention the crossing to the town. All the chroniclers say that Chiaha was on an island. The expedition remained at the town for about a month; the members, undoubtedly, explored the country in every direction in search of booty and pleasure; and they could not have mistaken for an island, the peninsula on which Rome is situated. Five days, the time consumed in travel from Chiaha to Coste, as located herein, was too short for the journey, when we consider the populous character of the country along the Coosa, and the abundance of food. We must, therefore, look for another route that fits all the facts as given by the chroniclers.

Let us go back to the 26th of May, when they camped in a plain in the mountains. Ranjel says: "There they crossed the river, wading up to their shins, by which later they were to depart in the brigantines they had made."[1] This stream is either the Cartecay or the Ellijay, probably the former, both tributaries of the Coosawattee river, though Ranjel evidently mistakes it for one of the tributaries of the Mississippi. They remained one day at this place, and the next day, the 28th, they passed the night in an oak grove, and the day following along a large stream, which they crossed many times.[2] The large stream was the Coosawattee, which they had now reached, and which they followed to Gausili. Further evidence that this stream is the Coosawattee is contained in Ranjel's words, "which they crossed many times." The Coosawattee, until it reaches Carters, the site of Gausili, flows through a gorge in the mountains; at numerous places, on such a stream, the way is barred, on one side or the other, by projecting bluffs and cliffs, and, in order to pass these obstructions, the expedition had to cross the stream "many times."

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[1] *Narratives of De Soto*, vol. 2, p. 104.

[2] *Ibid.*, p. 106.

The site of Gausili is at or near Carters, in Murray county, Georgia, where the Coosawattee emerges from the mountains, and is indicated on the map. The writer has stood on the deck of a steamboat, at Carters, and watched the river come tumbling down out of the mountains within a few hundred yards of where he was standing.

Elvas says: "He left Gausili and, after two days' travel, arrived at Conasauga. . . . Leaving Conasauga, he marched five days through a desert."[1] Desert, as used by the chroniclers, means the absence of villages where food could be procured. From Carters (Gausili) to the crossing of the Connasauga, the remains of village sites are plentiful, and it will be observed that this part of the route is not called a desert; but the route from the Connasauga to Rome, down the Oostanaula, is also marked by the sites of many villages, and had the expedition taken this route, there would have been no mention of a desert. Ranjel says: ". . . and, since all the way from Xuala (to Chiaha) had been mountainous, and the horses were tired and thin, and the Christians were also themselves worn out, it seemed best to tarry there (at Chiaha) and rest themselves."[2] Now, had they marched down the Coosawattee and Oostanaula rivers, from Carters (Gausili) to Rome, the route could not have been called mountainous, because these rivers flow through a beautiful valley. Ranjel says, further: "The next day, Thursday, they went along a large stream, near the river which they had crossed in the plain where the woman chief went off. It was now very large."[3] Now, if we turn to the route from Connasauga to the Chattooga river, and down that stream to Chiaha, the mystery is cleared and the route fits the description exactly. Ranjel says: "Monday, which was the last day of May, the Governor left Gausili and came with his army to an oak wood along the river, and the next day they crossed by Connasauga, and at night they slept in the open country."[1] It will be seen that De Soto "came with his army to an oak wood along *the* river," not along a river; therefore, they followed the Coosawattee to Connasauga and there left the river; because, the night following, "they slept in the open country," which means that there was no village at which they could camp; had they followed the river, villages would have been plentiful.

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[1] *Narratives of De Soto*, vol. 1, pp. 72–73.

[2] *Ibid.*, vol. 2, p. 107.

[3] *Narratives of De Soto*, vol. 2, p. 106.

From Connasauga to the Chattooga river, the expedition had to cross Lavender mountain and other ridges in an uninhabited or sparsely settled country. This is Elvas' "desert" and part of Ranjel's "mountainous way." An inspection of the map of Georgia and Alabama shows that, as they approached the Coosa river along the Chattooga, the route fits exactly the description as given by Ranjel, quoted above, when he says: ". . . they went along a large stream, near the river which they had crossed in the plain. . . ."

At the point where the Coosa and the Chattooga rivers approach each other, there is a large island, near Cedar bluff, in Cherokee county, Alabama, known as McCoy's island, the largest in the Coosa above Ten Islands shoals, and on this island was probably situated Chiaha, that illusive site that has puzzled investigators for so many years. The time from Gausili to Chiaha, six days, was ample in which to make the journey.

Another significant statement, by Ranjel, points to McCoy's island as the site of Chiaha. He says: "The next day, Saturday, the Spaniards crossed one arm of the river, which was very broad, and went into Chiaha, which is on an island in the same river."[2] Now, there are three large islands in this vicinity, but all are near the west, or right bank, except McCoy's island, which is near the east, or left bank; therefore, to reach it, the Spaniards had to cross the arm "which was very broad." No such statement could apply to the western crossings to the other islands nearby.

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[1] *Ibid.*, vol. 2, p. 106.

[2] *Narratives of De Soto*, vol. 2, p. 107.

McCoy's island is three thousand feet long and five hundred feet wide at the widest part. We must remember, however, that we are dealing with a description written nearly four hundred years ago, and since that time the island could have changed greatly. For example, the eastern or smaller branch of the river could have moved to the westward—such movements frequently occur—and thus have reduced the size of the island to such an extent that the site of Chiaha may now be wholly or partly on the eastern mainland.

Why did De Soto select this route to Chiaha, instead of that through the populous section bordering the Oostanaula and Coosa rivers? Because the latter was longer, and not in the direction he was at that time traveling to reach the coast. He had, also, been informed that food was plentiful at Chiaha, which proved to be true, and he decided to push forward by the shortest practicable route, which was the Indian trail between Gausili and Chiaha, part of the way along the Chattooga river. The route has been indicated, in a general way, on the map.

Monday, June 28, 1540, the expedition left Chiaha. The next morning "they had much labor crossing a river which flowed with a strong current."[1] This is Spring creek, which was probably swollen by rains that so frequently occur in this region in June and July. "Wednesday they passed over a river and through a village and again over the river and slept in the open country."[2] The first crossing was probably a branch of Terrapin creek; the second, Terrapin creek itself.

Five days after leaving Chiaha they reached Coste. The site of this town is at the head of Woods island, a large island in the Ten Islands shoals, containing about one hundred and eighty acres. Lock, Saint Clair county, Alabama, is near its lower end. There are evidences of a large town site near the head of the island, and many aboriginal objects have been found there; and, as yet, the surface only has been touched.

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[1] *Narratives of De Soto* vol. 2, p. 108.

[2] *Ibid.*, vol. 2, p. 109.

Now, for the proof that this is the site of Coste. It is just the right distance from Chiaha to have been reached in five days' travel, and this is the time given by Ranjel; it is the only large island between McCoy's and Densen's islands, the latter being twenty-two miles below. The distance from Chiaha to Densen's island is too great for five days' travel, now that the expedition was in a populous country where food was plentiful, and the table of distances shows they traveled leisurely through it. There was an Indian ford across the river at this island that was used by the early white settlers; and the writer, himself, has forded the river there, and can testify to the roughness of the bottom and the swiftness of the current. Now, hear what Ranjel has to say of this crossing, and it will be seen that his description fits exactly the conditions existing there at this day. "This village was on an island in the river, which there flows large, swift and hard to enter. And the Christians crossed the first branch with no danger to any of the soldiers, yet it was no small venture."[1] In other words, it was not a dangerous crossing; but, on account of the width (the branch they crossed being one-quarter mile wide), the swiftness of the current, and the roughness of the bottom, "it was no small venture." For "hard to enter," read "difficult of approach." The river bank at this old Indian ford is low, and the river itself is not "hard to enter"; but the approach is through a rugged, broken country, intersected in every direction by ravines and complicated with numerous outcroppings of rock ledges. "Difficult of approach" is undoubtedly the meaning of Ranjel's words.

The western arm or branch of the river is a narrow, shallow, insignificant stream, and, on leaving the island, the expedition crossed it to the west bank of the river. Ranjel says of the crossing: "Friday, July 9, the commander and his army departed from Coste and crossed the other branch of the river."[2] No mention is made of any difficulty in making this crossing; therefore, the "other branch of the river" was the narrow, western arm.

They passed the night on the west bank, and Ranjel says: "On the other side was Tali."[1] Elvas says: "The Christians left Coste the ninth day of July, and slept that night at Tali."[2] Ranjel declares that De Soto forced the Indians to take them across the river to Tali in canoes.[3] Only part of one day was consumed in travel from Coste to Tali, and it is, therefore, probable that Tali was situated at the mouth of upper Tallasahatchee creek, about one mile below the crossing of the Seaboard Air Line Railway, near Lock, Saint Clair county, Alabama, and opposite the site of Fort Strother, one of General Jackson's bases on the Coosa river, during the Creek Indian War of 1812–1814.

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[1] *Narratives of De Soto*, vol. 2, p. 109.

[2] *Narratives of De Soto*, vol. 2, p. 111.

We are now in a position to follow De Soto's course to Cosa from Chiaha. At the latter place he crossed to the east bank of the Coosa river, marched down that bank to Coste, then crossed to the west bank; marched part of one day down that bank; crossed again to the east bank at Tali; and continued down near the east bank to Cosa, where he left the river. The expedition left Tali Sunday afternoon, July 11, or Monday morning, July 12. "Monday they crossed a river and slept in the open country; Tuesday they crossed another river, and Wednesday another large river and slept at Tasqui."[4] The "rivers" they crossed Monday and Tuesday are Cane and Blue-eye creeks, in Talladega county, that enter the Coosa river between Ten Islands shoals and Choccolocco shoals. The "large river" they crossed Wednesday is Choccolocco creek, and the site of Tasqui is on the left bank of that large stream, in Talladega county, about one mile above its junction with the Coosa river, at the "Dickinson Mill Site," in Sec. 14, T. 17 S., R. 4 E., Huntsville Meridian.

Choccolocco creek would be called a river in any other state than Alabama. Its width at the place of De Soto's crossing is two hundred feet; its depth varies from a few feet on shoals to fifteen or twenty feet in pools. There is no other stream emptying into the Coosa river, on either side between Tali and Cosa, that could be called a "large river," even by the De Soto chroniclers, who named all streams rivers. Choccolocco creek is, therefore, the "large river" referred to by Ranjel.

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[1] *Ibid.*, vol. 2, p. 111.
[2] *Ibid.*, vol. 1, p. 80.
[3] *Ibid.*, vol. 2, p. 111.
[4] *Ibid.*, vol. 2, p. 111.

It is clear, from the accounts given in the De Soto narratives, that the expedition marched down the Coosa river from Chiaha to Coste and Tali. Elvas says: "Then taking the Chief (of Coste) by the hand, speaking to him with kind words, drew him with some principal men away from the town. . . . He told them that they could not go thence . . . until the sick men arrived whom he had ordered to come down the river in canoes from Chiaha."[1] Biedma says: "We left, following along the banks of the river, and came to another province, called Costehe."[2]

From Tali to Cosa they had to follow along the general course of the Coosa, though none of the narratives, nor does La Vega, mention such a stream. Along this part of the route villages were plentiful on the numerous creeks, and the route away from the river was more direct, and probably along the trail followed by the Indians. An examination of the United States Geological topographical maps, a list of which is given at the end of this paper, will give a much clearer idea of this part of the route than can be gotten from an inspection of the small-scale map herein (fig. 7).

On Friday, July 16, 1540,[3] the expedition entered Cosa. The chief, borne on a litter and surrounded by his retainers, came out to welcome De Soto. Even among savages, the rigid rules of court etiquette must be observed. La Vega says the Indians had habiliments of marten skins, which were fragrant.[4] The fragrance of the skunk did not appear to discommode the Indian wearers,—nor the Spaniards, as for that matter, for only passing mention is made of the circumstance.

In 1915, the writer visited the site of Cosa, and walked from the mouth of Talladega creek to the mouth of Tallasahatchee creek, a distance of one mile. The site of the village is in Talladega county, about three-quarters of a mile from the Coosa river. on Talladega creek, in Sec. 8, T. 20 S., R. 3 E., Huntsville Meridian, and about two miles above Childersburg, Alabama. At the mouth of the creek a ferry road has cut deep into the bank, thus exposing shell heaps buried some two feet under the surface. The depth of the shellheaps below the surface goes to show that the site is an ancient one.

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[1] *Narratives of De Soto* vol. 1, p. 79.

[2] *Ibid.*, vol. 2, p. 15.

[3] *Narratives of De Soto* vol. 1, p. 81; vol. 2, p. 112.

[4] *De Soto and Florida*, p. 374.

It is a beautiful country, in the vicinity of old Cosa, and in the sixteenth century, covered as it was then with a grand forest of hardwoods, it must have been "good to look upon," and one can understand the pleasure it excited in the chroniclers.

BIBLIOGRAPHY: MAPS OF THE U. S. GEOLOGICAL SURVEY

GEORGIA–SOUTH CAROLINA. Carnesville Sheet.

(Franklin county, Ga., north and south forks of the Broad river, in Georgia, junction of the Hudson river with the Broad, and probable site of Xuala.)

GEORGIA–NORTH CAROLINA–TENNESSEE. Ellijay Quadrangle.

(Cartecay and Ellijay rivers, and their junction to form the Coosawattee.)

GEORGIA–TENNESSEE. Dalton Sheet.

(Carters, Gausili, Murray county, Ga. The Coosawattee and Conasauga rivers.)

GEORGIA–ALABAMA. Rome Quadrangle.

(The mountainous country, Elvas' "desert," between the Conasauga and Chattooga rivers.)

ALABAMA. Fort Payne Sheet.

(Coosa and Chattooga rivers; McCoy's Island, site of Chiaha, near McClelland's ferry; Spring and Terrapin creeks.)

ALABAMA. Springville Sheet.

(Continuation of the Coosa river; site of Coste, on Wood's island, near Lock Three; Tali at Francis' ferry; Tasqui, one mile above mouth of Choccolocco creek.)

ALABAMA. Talladega Sheet.

(Continuation of the Coosa river; site of Cosa about three-quarters of a mile above the mouth of Talladega creek.)

BOOK REVIEWS

METHODS AND PRINCIPLES

Prolegomena to History. FREDERICK J. TEGGART. (University of California Publications in History, vol. 4, no. 3.) Berkeley, 1916.

This critical inquiry into methodology is of direct interest to anthropologists. While by a historian and concerned with the labors of historians, it is written in the attitude that "Anthropology and History differ only in so far as each represents the use of a special investigative technique." The scope of the work is indicated by the sub-title: "The Relation of History to Literature, Philosophy, and Science."

After an introduction summarizing the conflict of opinion whether history is a science, and a preliminary chapter given to a definition of the method of what is universally admitted to be science, the author considers the relation of historical investigation and historiography—those inquiries performed in the field of history that lead, in intention at least, to something akin to science, and the writing of history that is often viewed as an art. These two endeavors he finds to be linked chiefly by community of material, and to be sharply distinct. The historiographer "'selects' the facts to be included in his work in accordance with some personal localized view; and 'explains' events by the imaginative reconstruction of the character and motives of the participants." Though it is with nations and not with individuals that history is concerned, yet, as the group is only seen in named individuals, "there is an insistent tendency on the part of historians to lose the wider vision and follow the traditions of drama." Historiography is "no mere colorless product of scholarship. It is the mental reflection of the consciousness of national existence." "Intensity of personal feeling, inseparable from patriotism and politics," is what at once "gives history its specific quality" and "has remained the great obstacle to an historiographic art."

Section four deals with History and Philosophy, in which the thesis is upheld that historiography is ineradicably infected with a philosophical outlook, no less today when the idea of "the continuity of history" is heralded as the most fundamental principle of the discipline, than in the less systematized opinion of two millennia ago. This idea of continuity is only a modern formulation of the view which affiliates history with philosophy and thereby prevents its becoming a science. No proclamation that history is a science makes it such; nor has any verbal repudiation excluded the philosophical tinge from the history-writer's efforts. History indeed claimed to be a science, but plainly has not produced scientific results. Logicians have realized this situation; and, in England, escaped from the dilemma by admitting the claim and justifying it by finding in the "comparative method" a scientific element in the history-writer's work; whereas German philosophers have accepted the claim literally and announced that history was a science of a distinctive type. But the mere employment of the comparative method has obviously been insufficient to put history on a status at par with the recognized sciences; and, on the other hand, the erection of history into a new kind of science does not free this "science" from an essentially aesthetic or philosophical end and course. The writing of history still aims substantially at the purposes of Thucydides—not in the least to be decried, and productive now and then of works of timeless quality; but insuperably unscientific in value. If such practice is the only form which the results of historical research may take, this research is only ancillary to philosophy. "The subordination of investigation to historiography carries with it the subordination of investigation to philosophical ideas." "The future progress of historical investigation turns upon the possibility of scholars being able to free their work from the domination of historiography."

This brings the author to his final section on History and Evolution, which is of special interest to anthropologists in that it considers whether "the processes manifested in the activities of mankind" can be systematically intelligible; in other words, whether cultural phenomena are capable of definite resolution into their factors besides being described as facts. In a brilliant and original analysis of the logical method followed by scientists of the organic and inorganic, especially Hutton, Lyell, and Darwin, toward the working out of the concept of evolution, the author seeks to find at least some hints that may be useful to scientists concerned with civilization. Not that Darwin's theory is adaptable "to an evolution for which it was not designed"; but its essential method may be applicable to new problems. How this application may be brought about, the author does not pretend to predict; except for a hint that the evolutionistic method of science is after all the extension, to a wider content, of the "comparative method" which historians and anthropologists have not left unattempted; and for a caution that the evolutionary point of view may profitably center attention on the processes manifested in "fixity" as well as in those of "change," which have recently been accorded overwhelming interest.

There is then no positive conclusion; no royal road is pointed out. The little book remains within its purpose of being "Prolegomena." It deals, however, with problems which, whether we evade them or not, confront every anthropologist; and it outlines them with courage and a persistently critical spirit. That the work is written from the starting point of history, renders it almost the more stimulating to ethnologists, through imparting a freshness of presentation to an essentially identical task.

Two objections may be urged against the monograph. The first is the all too liberal use of citations. The quoted passages are apt, and constitute a convenient, perhaps even valuable, summary of opinions pronounced on the methodology of history; and their usefulness is enhanced by a full bibliography. But a constant skilful interweaving of citations with the text, and an occasional carrying on of the argument by successive juxtapositions of quotations, make difficulty for the reader, who soon finds himself wishing that the author had more sharply defined his own contribution by relegating incorporated matter to a visibly subsidiary place.

The other criticism concerns the actual recognition accorded to anthropology—its theoretical place in the author's opinion no ethnologist can cavil at. The anthropologists referred to are Maine, Lubbock, Tylor, Lang, Frazer, Hartland, M'Lennan, Myres, Marett,—and no others. Surely the day is past when this single closely knit series can be taken as representative of a study so diversified in aim, so unorganized in purpose even, as anthropology of today. Yet, if reproach is deserved, it is due far more to ourselves than to Professor Teggart. There is scarcely an ethnologist in this country, in France, or in Germany, who does not believe the narrow, simple method of the classic English evolutionary school to be sterile. That nevertheless only this school has been able to impress with its existence sincere students in allied branches, as well as the public, argues that it alone has consistently attempted or professed to solve wider problems. We may look down upon the solutions as superficial; but we must admit that the attempt has been earnest and gallant in spirit. Can we, who as the bulk of the profession hold aloof from this school, say as much for our endeavors?

A. L. KROEBER

Antike Technik. Sechs Vorträge. H. Diels. Teubner: Leipzig and Berlin, 1914. 140 pp., 50 figs., 9 pls.

H. Diels belongs to that sane and refreshing type of classical philologist that likes to cross the Chinese wall of the special field and by his broad outlook comes in contact with representatives of many other disciplines. In 1908 the edition of a new Greek work, ascribed to Melampus, and treating of divination from the throbbing or pricking of certain bodily organs (known to every one, for instance, from Shakespeare's Macbeth: "By the pricking of my thumbs, Something wicked this way comes") gave him occasion to study the same subject with the assistance of a staff of able collaborators, and to edit related books from Russian, Serbian, Bulgarian, Rumanian, Arabic, Hebrew, Turkish, Indian, German, English, and French,—a contribution to folklore of the first order.[1] His fruitful interest in the history of natural sciences and medicine is well known, and the attempt of such a high-standing authority to acquaint a larger public with some of the chief results of his science in concise and elegant form must be received with great satisfaction. He was mainly actuated by the desire to demonstrate by way of selected examples that classical antiquity, even in its technical achievements, is much more intimately associated with the modern world than the intervening middle ages, and that innumerable threads visible and unseen link these two worlds together. The struggle of engineering and natural science against humanism in the last century was based on a deplorable mutual ignorance and semi-culture of the two conflicting parties: the humanists, in the grip of a nebulous idealism, were unfamiliar with the material civilization of the ancients and unable to grasp its connection with our modern life, while the adversaries flatly rejected any reminiscence of our indebtedness to the ancients. This state of affairs has changed, for the classical philologists, though they may still belong to the best-hated species of mankind, a veritable *odium generis humani*, have now deeply penetrated into the realities of antique culture and pay as much attention to them as to its canon of beautiful forms and its ideal thoughts. The author's six lectures deal with science and technics among the Greeks; antique doors and locks; steam-engine, automaton, and taxameter; ancient telegraphy; ancient artillery; and ancient chemistry. Diels has been the first to reconstruct correctly the Homeric system of closing a door by means of lock and key, and his study of this subject is of great ethnological interest, as, for instance, analogies to the so-called *balanos* lock, which is found in Egypt, as early as the time of Ramses II (1292–25), still exist in China and Tibet. It might be worth the trouble to pursue this object in its distribution all over Asia. Our taxameter is modeled after the principle of Heron's hodometer, and certain modern automatic devices may well be traceable to ideas first conceived by Greek mechanicians. Carrier-pigeons were known in Hellas during the fifth century B.C., and were utilized by the Romans in beleaguered cities and for the transmission of sport news after races. As justly remarked by Diels, this practice was derived from the Orient. A Chinese author of the ninth century, it may incidentally be added here, reports that the Persians kept pigeons on board their ships and sent them back with letters to their homes over distances of several hundred miles (*Yu yang tsa tsu*, ch. 16, p. 6). Noah's dove, however, was not a carrier-pigeon, as asserted by Diels, but represents a distinct affair: it belongs to the category of land-spying birds dispatched by navigators in search of land, frequently mentioned in India and Babylonia, also by Pliny (VI, 22; see my notes in *T'oung Pao*, 1914, p. 11).

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[1] Beiträge zur Zuckungsliteratur des Okzidents und Orients, *Abhandlungen der preussischen Akademie*, two parts, 1908–09.

The chapter on the telegraphy of the ancients is full of interest. Long-distance communication was well developed by fire signals, a water telegraph, and torch signals expressive of the letters of the alphabet. In Aeschylus' *Agamemnon* we have the earliest example of a rapid transmission of a message from Asia to Greece: the conquest of Troy is signaled by huge bonfires from Mount Ida in Asia Minor by way of the island of Lemnos to Mount Athos, then flashed southward over Euboea into Boeotia and Mount Kithaerum, thence across the Isthmus to Epidaurus and ultimately to the castle of Mycenae. From Herodotus' statement that Mardonius after the battle of Salamis expected to send to Xerxes the news of the capture of Athens by fire-signals along the Aegaean islands to Asia, Diels rightly infers that such like institutions existed in Asia; in fact, they are alluded to in Babylonian texts. The most ingenious instrument of the ancients was the signal telegraph described and improved by the historian Polybius. Each station was formed by two battlemented turrets, each wall having five crenelles or embrasures, two feet distant from one another, in which torches could be placed. The system was based on a chiffre according to which the twenty-four letters of the Greek alphabet were distributed over five tables; in each table the letters were numbered in succession. Two operations were thus required for telegraphing a single letter, one turret serving to indicate the number of the table, the other, the number of the letter in question within this table. The letter K, for instance, was listed in the second table; accordingly, two torches were shown in the embrasures of the first bastion. K being the fifth letter of the second table, five torches were placed in the second bastion. It has been calculated that a dispatch of only three words, like "hundred Kretans deserted" would require a hundred and seventy-three torch-signals, and that the transmission could be accomplished within a half hour. It is obvious that this system contains the germ idea of electric telegraphy. It had no practical success, however, which Diels attributes to various causes, as the scanty range in the radiation of the torches, the great number of stations required, the complexity and costliness of the enterprise. It seems to me that the main cause for its failure must rather be sought for in the republican institutions of the Greeks, which lacked a centralized will-power and the strong hand of a ruler interested in the maintenance of such a service; for in all Oriental empires postal and intelligence organization was far superior to that of Greece. In ancient China, optical telegraphy by fire-signals was at a high stage of development as early as the third century B.C. Many of these ancient watch-towers have survived, particularly in Turkistan. They were erected in defence of the empire, the news of an invasion of nomadic tribes being at once signaled to the capital, two, three, or four firebrands in succession indicating the strength of the hostile forces. At the same time, and similarly as in the Persian empire, a staff of rapid couriers changing horses every fifth kilimeter or so was maintained. Marco Polo (chap. XXVI) and other mediaeval travelers have left us vivid descriptions of this magnificent postal system.

Artillery (by an irony of fate this word is derived from *artes*, "arts") was well known to the ancients, and from the descriptions of the historians and the writers on military matters the various types of antique cannon have been reconstructed and examined as to their range and effect. Remains of them have not survived, as they were chiefly constructed from wood; but missiles from sandstone, three to ten pounds in weight, and arrow-points have been unearthed in Spain on the site of the ancient Numantia, and are supposed to have been hurled into the city on the occasion of its gallant defense against Scipio the Younger in 133 B.C. For the first time we hear of the employment of artillery around 400 B.C. in Syracuse under Dionysus the Elder (I hardly think we can say with Diels that it was "invented" in that year; dates for inventions are usually fallacious). According to a theory of Diels, the ancient cannon was developed from the bow by way of the crossbow. With all their ingenuity the ancients have nothing to oppose to the Chinese repeating crossbow with a magazine holding up to ten darts which may be shot consecutively and rapidly, the darts sinking through their own weight,—mentioned in the Annals as early as 211 B.C.,—the prototype of our magazine rifle. Diels frankly admits that the modern development of weapons is not directly connected with Hellenic tradition; but "an imperceptible transition is visible in the so-called Greek fire derived from Greek tradition in its relation to modern fire-weapons." Indeed, he heralds the Greek fire as the precursor of gunpowder, without any regard to the well-founded claim of the Chinese to this invention and the admission of the Arabs that it was received by them from the Chinese. It is unproved that the Greek fire, first mentioned by Marcus Graecus in his *Liber ignium* toward 1250, has any connection with ancient Greece; moreover, it was a combustible or incendiary, but not an explosive like gunpowder. In China we can trace a rational development of gunpowder from the humble firecracker (known in the sixth century of our era), which was originally employed in religious ceremonies, to the launching of fiery projectiles in warfare as early as the twelfth century and the full development of fire-weapons under the Mongols in the thirteenth and fourteenth centuries. In Java, Sumatra, and the Philippines fire-arms were known and used prior to the arrival of Europeans. It is therefore out of place to say that as early as the beginning of the fifteenth century cannon advanced from Europe to China (p. 101). An Arabic treatise on gunpowder written in 1249 and preserved in the Library of the Escurial, styles saltpetre "Chinese snow" (*thelg-as-Sīn*) and the rocket "Chinese arrow" (*sahm khatāī*); saltpetre was first exported by the Arabs from China, where it occurs as a natural efflorescence of the soil in Szechuan, Shansi, and Shantung. Arabic writers mention fire-arms as far back as 1312, and in 1323 they were used at the siege of Baza. It is certainly due to Moorish influence when they make their first appearance in Spain. In the opinion of Diels, "this progress (?) is due neither to the Arabs whose claims are certainly unjustifiable, nor to the Chinese, but to the Germans," because the Germans were regarded by Byzantinians and Italians as the inventors of this new "barbaric" technique, and because in the fourteenth and fifteenth centuries the Germans exclusively possessed a literature on artillery, and German gun-smiths were everywhere in demand. All this is imaginary or inconclusive. The Germans may have had their share in the further development of fire-arms, but crediting them with their "invention" and even with the invention of gunpowder is a misstatement of facts.

In his discourse on chemistry Diels rejects the current etymology of this term from the Egyptian. He adopts the forms *chymes* and *chymeia* as a basis and derives them from Greek χύμα (metal-cast), metal-casting being in the center of antique chemical technology. A very interesting extract is given from the *Papyrus Holmiensis* in Upsala concerning Egypto-Hellenistic alchemy. At the end the author comes back to his former theory that alcohol should have been known to the Greeks, and was to a larger extent manufactured toward the close of the thirteenth century after a recipe of probably Greek origin. The article of J. Ruska in which this opinion is convincingly refuted should at least have been mentioned; also it would be interesting to know what Diels' attitude toward his opponent's arguments is.[1] The reviewer sides with Ruska and E. v. Lippmann in the opinion that the isolation of alcohol is not due to the Greeks or the Arabs, but a discovery made in Europe toward the close of the middle ages. It is justly emphasized by Ruska that with all respect for Alexandrine and Arabic science we are compelled to admit that the age of discoveries begins in the West at an earlier date than is usually assumed, and that we are not justified in contesting to the closing middle ages the discoveries then mentioned for the first time, although frequently under a false flag. The middle ages were not quite so dark and backward as believed by Diels and many others. Though we respectfully dissent from the author as to his result in the alcohol problem, we gladly concede that his study contains a great amount of new and useful information, and in the same manner as the present volume, is an important contribution to the history of science.

B. LAUFER

ASIA

Indian Shipping. A History of the Sea-Borne Trade and Maritime Activity of the Indians from the Earliest Times. RADHAKUMUD MOOKERJI. Longmans, Green and Co: London and New York, 1912. XXVIII, 283 pp.

Books move slowly these days. A copy of Professor Mookerji's work reached me only in August, 1916. The history of navigation and shipping is a subject of primary importance for the study of early international relations: especially in the sphere of the Indian Ocean, tribal migrations, ethnic movements, commercial conditions, intellectual exchanges, can be fully comprehended only by close attention to the rôle which the high seas have played in connecting India with Africa, the Archipelago, and the Far East. Mookerji is the first to give us in a handsomely printed volume a coherent survey of Indian navigation from earliest times down to the end of the Moghul period, and his account is interesting and readable from beginning to end. While it is chiefly based on well known English translations of Indian sources, the author has succeeded in finding a new Sanskrit text on ship-building by Bhoja, in which some information on various types of boats is given. A task which the ethnologist would like to see performed in this connection—a detailed study of the present-day types of Indian vessels—is unfortunately not attempted; but ships figured in ancient Indian sculpture, painting, and coins are studied to some extent and accompanied by good illustrations.

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[1] Compare H. Diels, "Entdeckung des Alkohols," *Abhandlungen der preussischen Akademie*, 1913, no. 3; J. Ruska, "Ein neuer Beitrag zur Geschichte des Alkohols," *Der Islam*, vol. IV, 1913, pp. 320–324, and "Alkohol and Al-kohl, zur Geschichte der Entdeckung und des Namens," *Aus der Natur*, vol. X, pp. 97–111.

Bhoja recommends that no iron should be used in the holding or joining together of the planks of sea-going vessels, for the iron will inevitably expose them to the influence of magnetic rocks in the sea, or bring them within a magnetic field and so lead them to risks. Mr. Mookerji thinks that this warning is worth carefully noting, and that this rather quaint direction was perhaps necessary in an age when Indian ships plied in deep waters on the main. There is, of course, no reality involved here, but we face the well known legend of the Loadstone Mountain which is believed to attract toward it ships wrought with iron and to smash them to pieces. René Basset has treated this subject in an interesting manner under the title "La Montagne d'Aimant" (*Revue des traditions populaires*, 1894, pp. 377–380). Jordanus, Montecorvino, Friar Odoric, and Marco Polo have described these vessels that have no iron in their frame and are only stitched together with twine (see Yule, *Cathay*, new ed., vol. II, p. 113). In all likelihood, the legend was carried to India by the Mohammedans.

From the number of passengers given for ships in the Buddhist birth-stories (*jātaka*) Mookerji is inclined to draw inferences as to the size of the ships. This conclusion is not admissible, as such numbers as 500 or 800 are quite typical in Buddhist stories and mean nothing more than any indefinite large number. If the boat carrying the Buddha to the Sea of the Seven Gems was loaded with 700 merchants besides himself, it is evident that the figure 700 is symbolically chosen to correspond to the Seven Gems.

The discourse on the traditions of the gold-digging ants (pp. 96, 97) lags somewhat behind the times. This legend is not Indian in its origin, but, as shown by the reviewer on a previous occasion, arose in Central Asia, where it was anciently known, and was diffused from there to India. At any rate, the fantastic explanations of Ball and Schiern, which Mookerji accepts in good faith, are far from the point.

The contrivance on the boat reproduced from the sculptures of Borobudur (plate opposite p. 46, no. 1) is not, as explained, a compass, which was not yet in existence at that time, but is simply a reel or pulley, over which the ropes for towing the vessel are run, as is plainly demonstrated in figure 5. The same device is placed both on the prow and stern of the boat (fig. 3).

The author throughout speaks of India as a unit, and ardent Indian patriot as he is, he tells us a great deal of Hindu imperialism in ancient times and India's command of the sea for ages. Here again the ethnologist will pause to raise the question what the share of the manifold tribes of India was in the navigation of the ocean, what the Aryans owe to the Dravidas, and what foreign influences on Indian boatgear and methods of shipping and navigating may have been in operation. The ancient Egyptians, Persians, Malayans, Cambodjans, Chinese, and Arabs were all sailing the Indian Ocean, and it is difficult to believe that these various peoples should not have learned from one another. This subject remains to be studied at close range. One lesson is worthy of being retained, and this is that the idea of an exclusive mastery of the sea and the destruction of rivals never entered into the minds of any of those nations, and that they plodded along one beside the other in peaceful competition till—the first Europeans arrived. The Mediterranean spectacle of Carthage and Rome was not repeated along the expanse of the Indian Ocean.

In his conclusion, Mookerji laments the want of a fully developed Indian shipping at the present time, and makes an appeal to the Government, and all who are interested in the material progress of India, "to be fully alive to the importance and necessity of reviving and restoring on modern lines a lost industry that rendered such a brilliant service in the past, and with which are so vitally bound up the prospects of Indian economic advancement."

The author should by all means be encouraged to continue his meritorious studies, but they should be founded on a broader basis. His knowledge, for instance, of the history of Java, Farther India, and China is not up-to-date, chiefly owing to his failure to read French literature. As to the Arabic and Persian sources, G. Ferrand's *Relations de voyages et textes geographiques relatifs à l'Extrême-Orient* must be consulted. The failure to utilize Chao Ju-kua in the translation of Hirth and Rockhill, where numerous references to shipping and sailing could have been gathered, is a serious drawback. Chao Ju-kua is referred to but once (p. 170), and wrongly designated as a Chinese traveler of the thirteenth century. He did not travel at all, however, but collected his notes on foreign trade and peoples, while stationed as Inspector of Maritime Trade at the port of Tsʻüan-chou in Fu-kien Province.

B. Laufer

Guide au Musée de l'École française d'Extrême-Orient. H. Parmentier. Hanoi: Imprimerie d'Extrême-Orient, 1915. 136 pp. 32 pls.

Few modern seats of learning have had a more glorious history than the École française d'Extrême-Orient, founded in 1898 at Hanoi, French Indo-China, by Governor General Doumer at the instigation of the Institut de France. In the first line the École is a research institute entrusted with the task of exploring the archaeology and ethnology of Indo-China, which, owing to the peculiar cultural position of this region, necessitates close study of both India and China and the interrelations of the two countries. The publications of the School are hence devoted to indology as well as sinology, the center of gravitation being directed toward Farther India. The annual Bulletin, of ambitious size, fifteen volumes of which have been issued, has earned the well-deserved reputation of being the leading and most solid periodical in all scientific matters relating to the East: every contribution contained in it is of importance and signals a decided advance in our knowledge of the subject. Besides this Journal, the School has published twenty special volumes concerned with philology, bibliography, numismatics, and archaeology,—all fundamental works of permanent value. The practical duty imposed upon it is to preserve the historical monuments of Indo-China, to propose the necessary measures for their protection, and to watch the execution of the orders insuring their safeguarding. Two precious instruments of work are at the disposal of the institution,—a library particularly rich in manuscripts and rubbings of inscriptions, and an ethnographical and archaeological museum. Not many among us may know that the city of Hanoi is even favored with two museums, the other being the Musée Agricole et Commercial, which harbors selected examples of the modern industrial art of Indo-China. The museum of the École is under the able guidance of H. Parmentier, an architect and art student of note, who directs the archaeological work of the institution. We owe to him an *Inventaire descriptif des monuments čams de l'Annam* (with a separate volume of plates), a monograph on the Temple of Vat Phu, and two ingenious studies bearing on the interpretation of architecture in the bas reliefs of ancient Java and Cambodja. A clear-minded scholar and an artist with a vision, he has stamped his personality on the arrangement and interpretation of the collections in the Museum of Hanoi, to the treasures of which we are introduced by him in a well written little guidebook. The prehistoric archaeology of Indo-China is well represented. Any palaeolithic types of stone implements (*pierre taillée par éclats*) have not yet been discovered in its soil; the prehistory opens with an epoch of polished stone, in the same manner as in China. Also in Annam the folklore interpretation of stone implements as thunderbolts is prevalent, and the same belief attaches to prehistoric bronze hatchets; in southern Annam a tradition is current that the latter should be produced only in places where a living being has been struck, the metal being silver or gold in the case of a man. The stone artifacts of Tonking, in general, are carelessly wrought and of mediocre material, save numerous small pieces executed in phtanite. The interpretation of the spade-shaped utensils from Tortoise Island as mattocks for hoeing the rice fields is doubtless correct (compare the reviewer's *Jade*, pp. 47, 78). No information, unfortunately, is given on the potsherds of the stone age of Annam. The few observations made on the prehistoric pottery of Laos and Cambodja whet our appetite for more and make us wish for a full publication of this important material. With a certain degree of chagrin we note that the present inhabitants exploit the shell heaps of Samrong-sen on their own hook for the purpose of extracting an excellent lime, and find there quantities of stone implements and shell ornaments. This reckless unprofessional digging should be stopped; these shell heaps ought to be systematically explored and to receive the same governmental protection as the historic monuments. The Museum owns a very large and interesting series of bronze hatchets from Tonking, abounding in unusual forms and technically of great perfection. It appears plausible that metal in this area was largely reserved for weapons, while stone continued to be utilized for household utensils; and this conclusion is supported by the observation that no stone arrowhead or spear has ever been found in this region. The engraving on a bronze fragment of two stags with large antlers under a dugout with two rowers is emphasized as a unique specimen of the prehistoric art of Indo-China.

Coming to historical times, the arts of Cambodja, Laos, Siam, Burma, Annam, China, and Tibet, with minor sections bearing on Corea and

Japan, are represented by well selected material. A good sketch of the prominent figures of the Lamaic pantheon is inserted. The unexplained female statuette (plate XXIX and p. 91) appears to have formed part of a triad, and to represent one of the consorts of Padmasambhava, probably Mandārava; at any rate, it is the type of an Indian, not a Tibetan woman. A piece of evidence of Japanese relations with Indo-China is presented by a Japanese sword guard found at Angkor Vat. Some terra cotta statuettes discovered in the marshes or rice fields of villages in the proximity of Hanoi are said to be relatively modern and of Canton manufacture; but one representing a lion with human head is regarded as older than the others. This type is well known among the T'ang clay figurines from Shen-si Province, being doubtless modeled after an Iranian prototype.

From plate XIX we glean with some consternation that a fine set of sacrificial bronze vessels of the K'ien-lung period is not cased. Chinese bronze, it is true, in general possesses a much greater power of resistance to atmospherical influences than Egyptian, Greek, or Roman bronze; but even if sheltered in a fairly air-tight case, it demands the constant, watchful care of a museum curator as to possible formation of malignant patinas. In the humidity of the Hanoi climate any open exhibition of whatever character would seem unsafe. We entertain the best wishes for the future growth and prosperous development of the Hanoi Museum, to which this attractive guidebook will assuredly win many new friends.

B. LAUFER

Chinese Clay Figures. Part I. *Prolegomena on the History of Defensive Armor.* BERTHOLD LAUFER. (Field Museum of Natural History, Publication 177, Anthropological Series, vol. XIII, no. 2.) Chicago, 1914. Pp. 69–315, 64 pls., 55 figs.

The basis of this series of studies, which the author modestly terms prolegomena, is afforded by certain ancient clay figures from the provinces of Honan and Shen-si—the region, in other words, where the old Chinese culture first took on its historical form. As is the case with all of Dr. Laufer's writings, the subject is treated with the utmost thoroughness, and light is thrown on it from almost every conceivable angle. In doing this the author has availed himself of his very wide knowledge of Chinese records and customs, and has based his conclusions upon evidence which seems incontrovertible. It is fortunate that Dr. Laufer is an ethnologist of thorough training and wide experience, for his consistent adherence to the modern anthropological point of view gives his work a quality which is lacking in much that has been done in the same field.

The first chapter of the work under consideration, entitled a "History of the Rhinoceros," is an excellent example of the thoroughness with which this writer treats his subject. Because the earliest defensive armor possessed by the Chinese was, according to him, of rhinoceros hide, the whole question of the former existence of that animal in China is gone into most exhaustively. Evidence is drawn not only from the ancient Chinese writings themselves, but from the records of Greece and Rome and of the Mohammedan world and mediaeval Europe, while the latest conclusions of zoology and palaeontology are carefully sifted for any data which may bear upon the subject.

Dr. Laufer's argument, in brief, is this: that in the archaic period (*i. e.*, down to the third century B.C.) the Chinese used armor made from the hides of two animals designated by the words *se* and *si;* that further, these words referred not to some large bovine form, as had heretofore been taken for granted, but to the rhinoceros; and, finally, that the *si* is to be identified with the existing two-horned Asiatic rhinoceros (*R. sumatrensis*), and the *se* with the single-horned animal (whether *R. unicornis* or *R. sondaicus* is not clear).

Previous writers have apparently treated this question only incidentally, adopting without adequate examination the statements of relatively late Chinese historians and lexicographers. It was the most natural thing in the world for earlier writers to compare the rhinoceros of their day with some such large bovine as, let us say, the water-buffalo; anybody who has seen both creatures must have been struck by their likeness, particularly when viewed tail-on. It would be equally natural, after the rhinoceros had become extinct and when it was remembered only as an animal "resembling a wild ox," to transfer its now ownerless name to the still surviving bovine. This process, as we all know, has been going on the world over; an almost exact parallel to the fate of the words *se* and *si* is the transference of the name "auerochs" to the European bison after the extermination of *Bos primigenius*. Dr. Laufer's argument seems most cogent and conclusive. There is, however, one minor point which seems to demand revision. On page 158 the suggestion is thrown out that the Chinese *si* may have been a descendant of the well known woolly rhinoceros (*R. tichorhinus*) which inhabited Siberia in Pleistocene times, and which, Dr. Laufer suggests, may have retreated gradually southward. To accept this, however, would be to controvert the much more plausible previous identification (p. 93) of the *si* with the existing *R. sumatrensis*, inasmuch as the place of *R. tichorhinus* is with the modern African group (*Atelodus*), and not with the much more primitive Sumatran animal.

One gains from this chapter not only much that is new regarding the former distribution of the rhinoceros, but also a most attractive picture of the archaic Chinese world of the first millennium B.C. We see not the congested and intensively cultivated country of today, but a region still affording shelter in its forests and jungles to numerous species of big game animals, and occupied by a warlike and chivalrous race with a highly developed bronze culture in many ways strangely reminiscent of that of Europe.

In chapter II, entitled "Defensive Armor of the Archaic Period," Dr. Laufer points out that until the termination of the Chou dynasty, in the middle of the third century B.C., armor was made exclusively of hide, and that iron armor and helmets were unknown until later. Several pages are devoted to a study of the nature of this armor, and most interesting comparisons are made with hide armor occurring in other culture areas, and particularly North America. The writer also shows that rhinoceros hide by no means went out of use with the introduction of metal armor, but that on the contrary it persisted as late as the T'ang period (A.D. 618–907). He then describes the two types of hide armor, the one (*kia*) a cuirass made in the form of a coat, the other (*kiai*) consisting of leather scales arranged like those of a fish. This most interesting chapter concludes by pointing out the striking coincidences between the development of defensive armor in the archaic epoch of China and that found in other ancient culture groups of Asia as well as in some primitive societies of the present.

The third chapter, "Defensive Armor of the Han Period," deals with the introduction of metal armor in China and the striking development in military organization which accompanied it. The congeries of loosely federated states which formed the China of the archaic period had been consolidated by the short-lived but exceedingly energetic dynasty of the Ts'in into a true centralized empire. This passed, at the very close of the third century B.C., under the dominance of the great house of Han. Marked advances now took place along all lines of culture, and both the political boundaries and the intellectual horizons of the Chinese people underwent a very remarkable expansion. In the field of warfare, Dr. Laufer points out, the old bow was superseded by the much more powerful crossbow, possibly adopted from the non-Chinese aboriginal tribes in the regions south of the Yangtse. At the same time the old bronze sword gave way to one of cast iron traceable to Siberia, while the chariot force which had formed the most important part of the armies of former times was replaced by regular cavalry.

Unfortunately the sculptured reliefs of the latter Han period, which give us such extraordinarily vivid and varied representations of the life of the time, reveal little regarding the defensive armor.

After a discussion of the shields of the Han warriors, and a reference to the life of the frontier guards who protected the northwestern border of the empire against marauding nomads, the author goes on to speak of the way in which metal armor first came into use, and the causes for its adoption. The probability is shown that it was copper which was first utilized, iron replacing it only during the first two centuries of our era (the latter half of the Han period). The author then points out what so many have failed to grasp, or at least to emphasize adequately—the great historical connections linking all Asia in matters of military art. This point is so important, and exemplifies so well Dr. Laufer's point of view in all his work, that his exact words deserve quoting. He says,

> No human invention or activity can be properly understood if viewed merely as an isolated phenomenon, with utter disregard of the causal factors to which it is inextricably chained. Every cultural idea bears its distinct relation to a series of others, and this reciprocity and interdependence of phenomena must be visualized in determining its historical position.

Dr. Laufer then goes on to show how the Persians, some time after their invasions of Greece, developed a true cavalry in the modern sense, utilizing shock tactics instead of distant skirmishing. The advantages and weaknesses of the new system are pointed out, as is the fact of its adoption by the Huns about the close of the third century B.C., just at the time, it will be recalled, when China passed under the sway of the powerful Han dynasty. Finally, the various stages are traced by which the Chinese themselves, out of the necessities of their secular strife with marauding neighbors, developed a body of true cavalry in emulation of their foes. The whole chapter affords a most interesting picture of certain aspects of the culture of the Chinese at a period when they had for the first time come in direct and conscious contact with the great civilized world of the west.

One hesitates to differ, even in minor points, with one who has so thoroughly mastered his subject as has Dr. Laufer. There is, however, a suggestion, early in this chapter (p. 201), which seems not quite convincing, although the point would not be worth dwelling upon did it not seem to disclose the existence of a certain convention among the artists of that day. Dr. Laufer remarks that many of the soldiers represented on the Han reliefs carry their shields in their right hands and their swords in their left, and suggests that this was done in order to rest themselves while fighting, by changing hands from time to time. Ambidexterity of this sort has certainly been known in the past; but its occurrence has invariably been so rare as to call for special mention. A careful study of the Han monuments suggests another explanation. In general, in these reliefs, movement seems to be from left to right; and when this direction is reversed, the attitudes of the personages seem also to be reversed. This is exactly what occurs, for example, in a relief (described by Dr. Laufer on p. 228) showing a battle with the Huns. Here in every case the archers advancing from the right hold the bow in the right hand and draw the arrow with the other, while their opponents, coming from the other direction, handle their bows in the normal manner. There can of course be even less question of changing hands with the bow than with the sword. This proposition appears to hold good for all the Han reliefs known to the present reviewer. Hence it would appear that this method of representation was due either to a feeling for symmetry, or else, perhaps more probably, to a desire to show the action of both hands; and the latter could not of course be accomplished if the arm held closest to the body were the one on the side away from the spectator.

The fourth chapter, devoted to a "History of Chain Mail and Ring Mail" begins with a definition of the two types; here, as always, Dr. Laufer's terminology is precise, logical, and consistent. The origin of chain mail is shown to have been in all probability in ancient Iran, appearing first during the Sassanian period. The evidence is then given for its introduction in China, where it seems first to have been made during the T'ang period (A.D. 618–907), from models derived from Samarcand. Ring mail, says Dr. Laufer, has never been much employed by the Chinese, but is ascribed by them to the Tibetans, who, as is known, had early relations with the Persians. The Tibetan sword and helmet, of Indo-Persian type, indicate the existence of this contact also.

Chapter v is a discussion of the "Problem of Plate Armor." This type is carefully differentiated from scale armor on the one hand and from sheet armor on the other, and is defined as consisting of flat rectangular laminae mutually lashed together, instead of being attached to a backing of leather or fabric, as is the case with scale armor. A generous tribute is paid to the work of Ratzel and Hough in investigating the distribution of plate armor in northeastern Asia and northwestern America, but it is shown that the conclusion arrived at by these investigators that Japan was the center of distribution of plate armor is untenable. The statement, however (p. 264), that there was no metal armor in

Japan prior to the end of the eighth century A.D., seems somewhat sweeping, although backed by the very great authority of Bashford Dean. At all events iron armor seems to have been associated in some instances with the dolmen type of burial, which tended to disappear after the introduction of Buddhism in the sixth century, and the discovery of blacksmiths' pincers of iron in deposits of that period would suggest that not all the metal objects found were importations from the continent. The point seems at least to call for further elucidation.

In the latter part of the chapter Dr. Laufer traces the history of plate armor during the T'ang period as illustrated by clay figures and carvings on stone. It is then followed through the Sung and Mongol periods, and the fact is pointed out that no fundamental change in plate armor has been made since the Mongol dynasty (A.D. 1280–1368), such alterations as have taken place pertaining only to style and ornamentation. A description follows of individual suits of recent date, and the chapter closes with a reference to the complexity of the technique of plate armor and the probability that a historical coherence exists between its various manifestations.

Chapter VI, upon "Defensive Armor of the T'ang Period," opens with a summary of the development of armor up to and including the T'ang epoch. Among the types mentioned is the curious paper armor which was then devised. The bulk of the chapter, a comparatively short one, is devoted to a discussion of the armor shown on certain clay figures of type originating in the Çivaitic worship of India and forming an unbroken series from early zoomorphic forms of Yama, God of Death, to figures of knights or champions of purely human character, without mythological connotations. It is pointed out that this type of image also occurs in Turkestan, and that therefore the armor which it wears, consisting essentially of metal plastron and dossière, is not Chinese in origin, but has some undefined relation to the sheet armor of the west. The chapter is brought to a close with a discussion of the so-called "lion armor" of the T'ang period, its probable nature, and the Indian-Buddhistic influence which it displays.

The closing chapter of the book, "Horse Armor and Clay Figures of Horses," gives us a historical résumé of horse armor, its Iranian provenance, and an account of the clay figures of horses and their riders from the provinces of Shen-si and Ho-nan, including a discussion of individual pieces and their significance.

Speaking of the work in its more mechanical aspects, there is very little fault to be found with it. Dr. Laufer's arrangement of his subject matter is logical and easy to follow, while his language is clear and makes pleasant reading. It seems, however, as though some of the matter dealt with in the footnotes might have been incorporated in the text with a distinct gain in the continuity of the thought. While the notes give an enormous amount of historical and other detail, all of great interest and having a direct bearing upon the question under discussion in the text, there is nevertheless a certain amount of distraction in referring to them so constantly. Also it would seem to have been better to have used the Chinese character along with the word transliterated, instead of merely referring to it by its number in Giles. The latter work is not always at hand, especially when one is traveling in the interior of China, and not the least of the many merits of Dr. Laufer's writings is their very great usefulness in field work.

The illustrations in the book are excellent, particularly the plates, which are carefully chosen and well executed and illustrate more adequately than is often the case the subject matter of the text. The proof reading has also been exceptionally well done, and only one or two small errors are to be noted. In the reference on p. 192, note 3, to Giles' *Biographical Dictionary*, p. 242 should be 212; while in that, p. 154, note, to Bushell, *Chinese Art*, vol. I, p. 119 should be 111.

On the opening page of this work Dr. Laufer says, "The second part of this publication will deal with the history of clay figures, the practice of interring them, the religious significance underlying the various types, and the culture phase of the nation from which they have emanated." It is to be hoped that this promised second part will not long be withheld.

C. W. BISHOP

OCEANIA

The Mythology of All Races. Vol. IX: *Oceania*. ROLAND B. DIXON. Marshall Jones Co.: Boston, 1916. Pp. XV, 364, 24 pls., 3 figs., map.

Professor Dixon's volume is a scholarly contribution that must prove enlightening to the general reader interested in myth and will be of real service to the ethnologist who wishes to get his bearings amidst the chaos of Oceanian literature. Under each of the headings of Polynesia, Melanesia, Indonesia, Micronesia, and Australia, there is a chapter on Myths of Origins and the Deluge, and another devoted to Miscellaneous Tales; where the material gives warrant an additional chapter is added, and a Summary closes each section. Naturally it is only possible to refer to a few points of interest within the limits of a review.

Commonly a genealogical and evolutionary type of cosmogonic myth has been considered characteristic of Polynesia. The author shows that this view requires qualification. The philosophical notion of a universe developing out of chaos through various intermediate steps into personifications of sky and earth by virtue of innate necessity is indeed highly typical of New Zealand, Hawaii and the Marquesas, but is much less developed elsewhere, being superseded in other groups by the creative type of origin myth. Indeed, even the Maori have variants in which Rangi and Papa, the Heaven Father and Earth Mother, are especially conspicuous, while the evolutionary element is absent. Professor Dixon here makes the interesting suggestion (p. 9) that the coexistence of different versions may be the result of the blending of two distinct peoples, the creative forms being characteristic of the autochthonous inhabitants and of the Moriori, while the evolutionary type was imported by later immigrants. However, it seems equally plausible to the reviewer to assume individual differences in point of speculative ability among a homogeneous population.

A comparison of all Polynesian myths (pp. 92–99) leads the author to some very interesting and in part unexpected results. Hawaii and New Zealand, notwithstanding their distance from each other, show remarkable affinity; Samoa has decidedly more kinship with the Cook Islands and New Zealand than with Tonga; and Central Polynesia constitutes something of a unit with varied affiliations. Melanesian elements occur in great number in New Zealand and Samoa but are weak in Hawaii and lacking in the Society and Cook groups. Parallels to Indonesian and Micronesian motives are especially common in Hawaii.

In Melanesia (including New Guinea) Professor Dixon distinguishes a Papuan from a genuinely Melanesian stratum (pp. 148–150). The former is characterized by the insignificance of cosmogonic features, the prominence of ghosts, and altogether by greater simplicity. The Melanesian myths lay greater stress on origin accounts, reveal peculiar fondness for cannibalistic motives and often reflect a sort of dualism in the juxtaposition of wise and foolish brothers. The Papuan layer shows hardly any relation to alien mythologies, but the Melanesian tales have connections with those from every other region in Oceania though the agreement between Micronesia and eastern Melanesia is particularly noteworthy.

For Indonesia the author calls attention to the total lack of Negrito material (p. 154). Eliminating the more recent Mohammedan influences, he recognizes a pre-Malay and a later Malay-Indonesian mythology. The older Indonesian type lacked strictly cosmogonic and trickster stories and had well-developed deluge myths. In the Malay-Indonesian mythology Hindu elements are prominent. Professor Dixon suggests that the primitive Malay folklore may be related to that of the Thai or Shan, the former occupants of southern China and northern Indo-China, and that the pre-Malay stock of Indonesian mythology may reveal affinity with that of the Mon-Hkmer (p. 244). These are unfortunately merely hints for future investigation.

Micronesia is mythologically closest to pre-Malayan Indonesia; there are also many parallels to Polynesian and Eastern Melanesian motives (p. 263).

In the discussion of Australia the author is obliged to direct attention to the absence of data for Tasmania and the western portion of the continent. He finds corroboration for Father Schmidt's linguistic classification of the natives in their mythology, which differs in the central and northern regions from that of the southeast. Cosmogonic myths are characteristic of the latter area while the more northern origin tales center largely in the appearance and activities of totemic ancestors. Oddly enough, resemblances to Melanesia, including such themes as the swan-maiden and the arrow chain, are decidedly more typical of the southeast (pp. 301–303).

Professor Dixon is fully aware of the tentative nature of many of his characterizations and historical inferences, resting as they often must on inadequate or uneven information. We must be grateful to him for blazing the trail in this field and for not yielding to the impulse, pardonable in the face of such difficulties as he had to contend with, to abstain from all wider generalizations. In one respect he might well have gone somewhat farther,—in the treatment of American parallels. For the benefit of his colleagues it would have been a highly desirable thing to present a systematic enumeration of such similarities, together with their provenience, and to discuss somewhat less summarily than he has done (p. 307) the conclusions that can legitimately be drawn therefrom.

From an external point of view this volume, if possible, excels its predecessors in the series. The map with its sharp demarcation of regional boundaries will be appreciated by most readers, but an index citing the principal motives would have been equally appropriate.

In the very full Bibliography, which includes many references to papers in Dutch publications, Krämer's *Die Samoa Inseln* is omitted through an obvious oversight.

ROBERT H. LOWIE

SOME NEW PUBLICATIONS

Boas, Franz. The Development of Folk-Tales and Myths. (The Scientific Monthly, October, 1916, pp. 335–343.)

—— Eugenics. (*Ibid.*, November, 1916, pp. 471–477.)

Boman, Eric. El Pucará de los Sauces; una fortaleza de los antiguos Diaguitas. (Physis, II, pp. 136–145, Buenos Aires, 1916.)

—— Los Primitivos Habitantes del Delta del Paraná, por el Doctor Luis Maria Torres. (*Ibid.*, pp. 334–338.)

—— Las Ruinas de Tinti en el Valle de Lerma. (Anales del Museo Nacional de Historia Natural de Buenos Aires, XXVIII, 1916, pp. 521–540.)

Bradley, Cornelius B. On Plotting the Inflections of the Voice. (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 5, pp. 195–218, pls. 1–5, October 11, 1916.)

Casanowicz, I. M. Paraphernalia of a Korean Sorceress in United States National Museum. (Proceedings of the U. S. National Museum, vol. 51, pp. 591–597, pls. 108–112.) Washington, Government Printing Office, 1916.

Crawford, M. D. C. Peruvian Fabrics. (Anthropological Papers of the American Museum of Natural History, vol. XII, part IV, pp. 105–191, 40 figs.) New York, 1916. Price $1.00.

Dixon, Roland B. The Mythology of All Races. Vol. IX: Oceania. Marshall Jones Co., Boston, 1916. Pp. XV, 364, 1 map, 3 figs., 24 pls.

Ferris, H. B. The Indians of Cuzco and the Apurimac. (Memoirs of the American Anthropological Association, vol. III, no. 2, April–June, 1916.) Pp. 55–148, 1 map, tables, pls. I–LX.

Handcock, P. S. P. The Archaeology of the Holy Land. T. Fisher Unwin, Ltd.: London, 1916. Pp. 373, 109 figs., 2 folding-plans, colored frontispiece.

Hatt, Gudmund. Moccasins and their Relation to Arctic Footwear. (Memoirs of the American Anthropological Association, vol. III, no. 3, pp. 147–250.) Lancaster, Pa., 1916.

Havemeyer, Loomis. The Drama of Savage Peoples. Yale University Press: New Haven, 1916. Pp. IX, 274.

Holmes Anniversary Volume: Anthropological Essays presented to William Henry Holmes in Honor of his Seventieth Birthday, December 1, 1916, by his friends and colaborers. Washington, 1916. Pp. VII, 499.

ten Kate, Herman. In Memoriam Dr. Johan Sasse Azn. (Tijdschrift van het Koninklijk Nederlandsch Hardrijkskundig Genootschap, 2e ser. dl. XXXIII, 1916, afl. 5, pp. 665–674.)

—— Een paar Woorden over Beschavingswerk onder Primitieven. (*Ibid.*, 2e ser. dl. XXXII, 1915, afl. 3, pp. 350–355.)

—— Mélanges anthropologiques. (L'Anthropologie, 1913, pp. 651–661. Tijdschrift van het Koninklijk Nederlandsch Hardrijkskundig Genootschap, 2e ser. dl. XXXII, 1915, pp. 235–245, 520–564.)

—— Psychologie en ethnologie in de koloniale politick. (Overgedruckt uit "De Indische Gids," Juni–Juli Aflevering 1916.) Amsterdam: J. H. de Bussy, 1916, pp. 84.

—— Oudheden (=Antiquities) *in* Encyclopaedie von Nederlandsch West Indie, ed. H. D. Benjamins and Joh. F. Snelleman, 1916, pp. 541–546.)

Kroeber, A. L. Arapaho Dialects. (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 3, pp. 71–138, June 28, 1916.)

—— California Place Names of Indian Origin. (University of California Publications in American Archæology and Ethnology, vol. 12, no. 2, pp. 31–69, June 15, 1916.)

Kissell, Mary Lois. Basketry of the Papago and Pima. (Anthropological Papers of the American Museum of Natural History, vol. XVII, part IV, pp. 115–264, 81 text figs.) New York, 1916. Price $1.50.

Marett, R. R. The Site, Fauna, and Industry of La Cotte de St. Brelade, Jersey. (Archaeologia, LXVII.) Oxford, 1916, pp. 75–118, pls. XIII–XIV, 48 figs.

Mead, Charles W. Conventionalized Figures in Ancient Peruvian Art. (Anthropological Papers of the American Museum of Natural History, vol. XII, part V, pp. 193–217, pls. I–VIII.) New York, 1916. Price $.25.

Sapir, E. Time Perspective in Aboriginal American Culture, A Study in Method. (Canada Department of Mines, Geological Survey, Memoir 90; no. 13, Anthropological Series.) Ottawa, 1916, 87 pp.

Sellards, E. H. Human Remains and Associated Fossils from the Pleistocene of Florida. (Eighth Annual Report of the Florida State Geological Survey, 1916, pp. 121–160, pls. 15–31, figs. 1–15.)

Uhlenbeck, C. C. Het identificeerend Karakter der Possessieve Flexie in Talen van Noord-Amerika. (Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeeling *Letterkunde*, 5e Reeks, Deet II.) Amsterdam: Johannes Muller, 1916, pp. 27.

Waugh, F. W. Iroquois Foods and Food Preparation. (Canada Department of Mines, Geological Survey, Memoir 86; No. 12, Anthropological Series.) Ottawa, 1916, 158 pp., 39 pls., 235 pp., 2 figs.

Wissler, Clark. Harpoons and Darts in the Stefánsson Collection. (Anthropological Papers of the American Museum of Natural History, vol. XIV, part II, pp. 397–443, 44 figs.) New York, 1916.

ANTHROPOLOGY AT THE NEW YORK MEETING WITH PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR 1916

BY GEORGE GRANT MACCURDY

THE eighteenth meeting (fifteenth annual) of the American Anthropological Association was held December 26–29, 1916, at the American Museum of Natural History, New York City, its scientific sessions being in affiliation with Section H, American Association for the Advancement of Science, and the American Folk-Lore Society.

The Council meetings were attended by Boas, Dixon, Goddard, Hagar, Hrdlička, Judd, Lowie, MacCurdy, Michelson, Peabody, Putnam, Sapir, Saville, Speck, Spinden, and Tozzer. In the absence of President Hodge, various members including: Tozzer, Dixon, Goddard, MacCurdy, and Wissler presided at the various sessions of the Council and of the Association.

REPORT OF THE SECRETARY

The Proceedings of the last annual meeting of the Association were published in the *American Anthropologist* for January–March, 1916. There has been no special meeting of the Association nor of the Council or the Executive Committee.

Two members have died during the year: Francis T. Hagadorn, January 26, 1916; and Dr. Robert Ansly, August, 1916. The death of Dr. C. E. Slocum, which took place June 7, 1915, not having been reported to the Secretary in time for his last annual report, is recorded here. Dr. Slocum was a Founder of the Association.

Of the 70 applicants for membership,[1] submitted by the Secretary, all were elected as follows: Amherst College Library, P. A. Bernard, Father Berard Haile, O. F. M., Nathan Bijur, Bureau of American Ethnology, California State Library, Wm. Churchill, James B. Clemens, M.D., Cleveland Public Library, College of Medicine Library (Univ. of Nebr.), Colorado State Teachers' College, Denver Public Library, Detroit Public Library, George F. Eaton, M. W. Ehnes, Enoch Pratt Free Library, Field Museum of Natural History, Free Public Library of Philadelphia, Manuel Gamio, A. T. Handy, B. B. Holland, Indiana

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[1] Full addresses are given in the list of members printed elsewhere in this issue.

University Library, Rev. Julius Jette, S. J., Kansas City Public Library, Kansas State Historical Society, Kansas State Normal School Library, T. Kincaid, G. Langford, Library Committee of Philadelphia, Library of Congress, Library of Parliament (Ottawa), Los Angeles Public Library, Miss M. A. Mallory, I. B. Millner, E. H. Morris, New Hampshire College Library, New Jersey State Library, New York State Library, Martinus Nijhoff, Ohio State University Library, L. Outhwaite, Peabody Institute, Pennsylvania State Library, Phillips Academy, Radcliffe College Library, Radcliffe Library (Oxford), A. P. Rice, Miss H. Roberts, Miss C. R. Romanow, St. Louis Public Library, Rev. S. S. Smith, State Historical Society, J. L. Stewart, L. R. Sullivan, Syracuse Public Library, T. W. Todd, University of California Library, University of Chicago Library, University of Melbourne, University of Minnesota Library, University of Missouri Library, University of Nebraska Library, University of Washington Library, University of Washington Museum, Victoria Public Library, Dr. J. J. Walsh, Miss B. Weitzner, Mrs. L. L. W. Wilson, Wisconsin State Historical Society, A. H. Wood, Jr.

This is about twice as many new members as were elected one year ago. The increase is to a large extent composed of subscribing institutions that have become members instead, thanks to the appeals of our new Treasurer and to the prospective amendments in our Constitution.

The Secretary is at the end of his thirteenth year of continuous service in the office (besides having served a year as Acting Treasurer). Cooperation of the members and the steady growth in membership have added much to the pleasure of the work. The Secretary feels, however, that his name should not be presented again for re-election.

As the fifteenth anniversary of the founding of the Association falls half way between this and our next annual meeting, and as his term of office has covered nearly the whole of the period, it might not be amiss for the Secretary to summarize here the record of events to date. There have been eighteen meetings; one Founders', two Special, and fifteen Annual meetings. Of these three were held in Washington, three in New York, two in Philadelphia, two in San Francisco (both special), and one each in Pittsburgh (Founders'), Saint Louis, Ithaca, Chicago, Baltimore, Boston, Providence, and Cleveland. Thirteen of the eighteen meetings have been held in affiliation with Section H of the American Association for the Advancement of Science.

The chief offices have been filled as follows:

President: W J McGee, 1902 Pittsburgh and Washington.
1903 Saint Louis, 1904 Philadelphia.

F. W. Putnam, 1905 San Francisco and Ithaca.
1906 New York.
Franz Boas, 1907 Chicago, 1908 Baltimore.
W. H. Holmes, 1909 Boston, 1910 Providence.
J. Walter Fewkes, 1911 Washington, 1912 Cleveland.
Roland B. Dixon, 1913 New York, 1914 Philadelphia.
F. W. Hodge, 1915 San Francisco and Washington.
1916 New York.

Secretary: George A. Dorsey, 1902 (half year), 1903.
Alfred L. Kroeber (declined on account of distance).
George Grant MacCurdy, 1904–1916 inclusive (elected at a Council Meeting in New York, March 28, 1904).

Treasurer: Roland B. Dixon, 1902 (half year), 1903.
B. T. B. Hyde, 1904–1915 inclusive.
George Grant MacCurdy, Acting Treasurer for 1915.
Neil M. Judd, 1916–.

Editor: F. W. Hodge, 1902–1910, 1911–1914.
J. R. Swanton, 1910–1911.
Pliny E. Goddard, 1915–.

Report of the Treasurer

Receipts and Disbursements, January 1, 1916, to December 31, 1916

Receipts

Balance from the Acting Treasurer, G. G. MacCurdy			$ 212.63
Anthropological Society of Washington:			
Balance due January 1, 1916	$191.08		
American Anthropologist, vol. XVII, no. 4	46.26		
American Anthropologist, vol. XVIII, nos. 1–3	145.34	$382.68	
Less amount still unpaid		47.25	335.43
American Ethnological Society:			
American Anthropologist, vol. XVII, no. 4		61.19	
American Anthropologist, vol. XVIII, nos. 1–3		180.27	241.46
Annual membership dues:			
Account of 1914		73.00	
" of 1915		144.50	
" of 1916		1,145.00	
" of 1917		86.00	1,448.50
Annual subscriptions to *American Anthropologist:*			
Account of 1914	$ 8.00		
" of 1915	24.61		
" of 1916	325.00	357.61	
Less amount still unpaid		4.05	353.56
Sale of publications		239.86	
Less amount still unpaid		34.47	205.39
Authors' reprints at cost			25.73

Gifts to the Association:		
Prof. Henry Montgomery	4.00	
Dr. George Grant MacCurdy	10.00	14.00
Illustrations, *Memoirs*, vol. III, pt. 2		373.36
Miscellaneous, including overcharges		39.29
Total receipts		$3,249.35

Disbursements

Printing and distribution of publications:			
Amount unpaid, Jan. 1, 1916	$1,747.60		
Less amount unpaid, Dec. 31, 1916	1,392.60	$ 355.00	
American Anthropologist, vol. XVII, no. 4	331.04		
American Anthropologist, vol. XVIII, nos. 1–3	1,113.96	1,445.00	
Memoirs: Title page and index, vol. II	30.19		
Memoirs, vol. III, no. 1	100.31	130.50	$1,930.50
Illustrations for publications:			
American Anthropologist, vol. XVII, no. 4	8.99		
American Anthropologist, vol. XVIII, nos. 1–4	203.32	212.31	
Memoirs, vol. III, nos. 2 and 3		431.79	644.10
Expenses of the Editor			226.56
Expenses of the Secretary			120.96
Expenses of the Treasurer			93.45
Refund for overcharges			24.52
Miscellaneous			13.51
Total disbursements			3,053.60
Balance in Washington Loan & Trust Co.			195.75
			$3,249.35

NEIL M. JUDD,
Treasurer

The accounts of Neil M. Judd, Treasurer, have been examined and found correct.

Signed:
TRUMAN MICHELSON,
WALTER HOUGH,
Auditing Committee

WASHINGTON, D. C.,
January 5, 1917

REPORT OF THE EDITOR

At a meeting of a committee appointed at the Washington meeting to act, it was decided that the *Anthropologist* should be reduced in size to about 600 pages and that the longer articles should appear quarterly in the *Memoirs*. The third number of the current volume, 18, ends with page 453. For special reasons the fourth number now being paged will somewhat exceed the allotted 150, making a total of about 610 pages.

Three numbers of the *Memoirs* have been issued this year. The final number of volume 2 consists of an article entitled "Pottery of the Pajarito Plateau and of some adjacent regions in New Mexico" by A. V. Kidder. The first number of volume 3, beginning the quarterly issues, was devoted to an article by H. K. Haeberlin, "The Idea of Fertilization in the Culture of the Pueblo Indians." The second number consisting of 148 pages and 60 plates gives the physical results of the Yale-National Geographic Peruvian Expedition, a paper prepared by Dr. H. B. Ferris. The expense of publishing this number is being defrayed by the National Geographic Society. The third number now ready for mailing is a monograph entitled "Moccasins and their Relation to Arctic Footwear" by Dr. Gudmund Hatt.

The policy of the last year of receiving the contributions from anthropologists of foreign countries whose means of publication are temporarily interrupted has been continued.

Dr. Lowie, as in the past, has had entire charge of the Review section and Dr. Swanton has looked after the interests of the anthropologists in Washington.

The character of the articles appearing in the *Anthropologist* and the *Memoirs* is almost wholly determined by the spontaneous activities of the working members of the Association. They reflect rather fairly the present views and interests of American anthropology.

PLINY E. GODDARD,
Editor

AMENDMENTS TO THE CONSTITUTION

Amendments to the Constitution proposed by Boas, Hodge, and MacCurdy and referred by them to the Council at the Washington meeting, and duly announced in the notice calling the New York meeting, were approved by the Council and adopted by the Association as follows:

ARTICLE III.—*Membership*

By adding "or institutions" to SECTION 2, so that when amended the SECTION will read; "Persons or institutions interested in Anthropology may be elected on nomination of two members of the Association, and on payment of dues shall become Members of the Corporation, with full rights of voting and holding office."

By substituting for "member" the words: "person but not a library or other institution," to SECTION 3, so that when amended it will read: "Any person but not a library or other institution may become a Life Member on payment of $100 at one time."

ARTICLE IX.—*Finances and Property*

By omitting: "for members of the American Association for the Advancement of Science and of the American Folk-Lore Society, the annual dues shall be $5.00" from SECTION 3, so that when amended it will read: "The annual dues of members shall be $6.00; for members of local affiliated societies, that subscribe for the *American Anthropologist* for each of their members and which have adopted the *American Anthropologist* as their official organ, the dues shall be $1.00."

Elsewhere in this issue will be found the Articles of Incorporation of the Association and the Constitution as revised to date.

The Council took affirmative action on the following:

1. That members of the American Ethnological Society become members of the American Anthropological Association upon the certification of the fact of their membership by the Secretary of the Ethnological Society to the Secretary of this Association.

2. That the expenditure of the Permanent Fund be ratified as a loan from the General Fund.

3. That a Committee consisting of Judd, Hodge, and Michelson be appointed to investigate the General Fund of the Association and report at the next meeting.

4. That the Secretary report on the question of redundancy in ARTICLE II of the Constitution. (His report is that it contains no redundancy not found also in the Act of Incorporation.)

5. That the Secretary send the thanks of the Council to Mons. Léon Coutil for the gift of the three bound volumes of his collected papers. Also that the Secretary be empowered to dispose of volumes received on behalf of the Association and report to the latter as to the details of said disposals.

6. That the program of the next annual meeting be printed independently.

7. That the Treasurer be entrusted with the Archives of the Association.

8. That the Secretary and Treasurer be empowered to destroy letters, papers, etc., which in their judgment are not of permanent value to the Association.

9. That the Act of Incorporation and the Constitution as revised to date be reprinted (see p. 101).

10. That a Committee consisting of the President, Secretary, and Treasurer be empowered to decide on the place of the next annual meeting of the Association.

The Secretary was instructed to convey the thanks of the Association to the American Museum of Natural History and to the local members for facilities so generously supplied. With special remembrance will those present look back upon the dinner at Hotel Majestic and the daily luncheons at The Endicott. The luncheon on the closing day was in honor of Professor Franz Boas, marking as it did the thirty-fifth year of his doctorate. Short speeches were made by Goldenweiser (who presided), Tozzer, Lowie, MacCurdy, Sapir, and Goddard. To these collectively Professor Boas responded.

The following telegram was sent to President Hodge:

> The American Anthropological Association has voted unanimously that the Secretary send you greetings and express its regrets that illness has prevented you from being present and presiding over its deliberations.
>
> (Signed) G. G. MacCurdy.

The Secretary is in receipt of the following telegram in response:

> I greatly appreciate your telegram. Please extend my cordial greetings and every good wish to the officers and members of the American Anthropological Association. I greatly regret not being with you this time, but hope for better things in the future
>
> (Signed) F. W. Hodge.

On motion of Professor Boas, a Committee consisting of Boas, Goddard, and Farabee was appointed to draft resolutions on the retirement of George Grant MacCurdy from the post of Secretary of the Association. The Committee reports as follows:

> Whereas Dr. George Grant MacCurdy has served the American Anthropological Association for thirteen years as Secretary, and has during this time, by his rare devotion to the interests of the Association, contributed a large share towards building up the Association, and through it towards anthropological work in our country: *therefore* be it
>
> *Resolved*, That the American Anthropological Association express their sincere thanks to Dr. George Grant MacCurdy for the signal services which he has rendered to the Association.
>
> (Signed) Franz Boas,
> Pliny E. Goddard,
> Wm. Curtis Farabee,
> *Committee on Resolutions.*

January 2, 1917.

The Chair appointed a Committee on Nominations consisting of Goddard, Dixon, and Sapir, whose report was accepted by the Association, the election of officers resulting as follows:

President: Alfred L. Kroeber, Affiliated Colleges, San Francisco.
Vice-President, 1917: George B. Gordon, University of Pennsylvania.
Vice-President, 1918: B. Laufer, Field Museum of Natural History.
Vice-President, 1919: John R. Swanton, Bureau of American Ethnology.
Vice-President, 1920: George Grant MacCurdy, Yale University.
Secretary: Alfred M. Tozzer, Harvard University.
Treasurer: Neil M. Judd, U. S. National Museum.
Editor: Pliny E. Goddard, American Museum of Natural History.
Associate Editors: John R. Swanton, Robert H. Lowie.

Executive Committee: The President, Secretary, Treasurer, and Editor (*ex-officio*), Clark Wissler, Edward Sapir, W. C. Farabee.

Council: F. Boas, W. H. Holmes, J. W. Fewkes, R. B. Dixon, F. W. Hodge, A. L. Kroeber, George B. Gordon, B. Laufer, John R. Swanton, G. G. MacCurdy, A. M. Tozzer, Neil M. Judd, P. E. Goddard (*ex-officio*); W. C. Mills, H. Montgomery, C. B. Moore, W. K. Moorehead, E. K. Putnam, C. Peabody, C. C. Willoughby, T. Michelson, A. B. Skinner, M. H. Saville, E. W. Hawkes, (1917); Alice C. Fletcher, C. P. Bowditch, S. Culin, R. H. Lowie, C. H. Hawes, E. Sapir, N. C. Nelson, H. Bingham, J. A. Mason, H. Haeberlin, E. W. Gifford, Elsie Clews Parsons, (1918); A. E. Jenks, S. A. Barrett, W. Hough, A. Hrdlička, B. T. B. Hyde, C. Wissler, F. G. Speck, A. A. Goldenweiser, E. A. Hooton, A. V. Kidder, F. C. Cole, L. Spier, (1919); Byron Cummings, W. C. Farabee, G. G. Heye, H. J. Spinden, C. M. Barbeau, W. D. Wallis, A. B. Lewis, S. Hagar, Miss H.N. Wardle, Theodoor de Booy, F. H. Sterns, S. K. Lothrop, (1920).

The incoming President, Professor Alfred L. Kroeber, has appointed the following committees:

Committee on Program: A. M. Tozzer (chairman), Roland B. Dixon, Alice C. Fletcher, Ales Hrdlička, G. G. MacCurdy, Berthold Laufer, Clark Wissler.

Committee on Finance: Charles Peabody (chairman), Edward E. Ayer, Charles P. Bowditch, William H. Furness, George G. Heye, Clarence B. Moore, Homer E. Sargent.

Committee on Policy: Franz Boas (chairman), Roland B. Dixon, J. Walter Fewkes, W. H. Holmes, Robert H. Lowie, George G. MacCurdy, A. M. Tozzer.

Committee on Publication: A. L. Kroeber (chairman *ex-officio*), Hiram Bingham, Stewart Culin, P. E. Goddard (secretary *ex-officio*), A. A. Goldenweiser, G. B. Gordon, Walter Hough, Neil M. Judd, F. W. Hodge, Berthold Laufer, E. Sapir, M. H. Saville, John R. Swanton, A. M. Tozzer.

To represent the Association on the Council of the A. A. A. S.: W. H. Holmes and Franz Boas.

ADDRESSES AND PAPERS

The retiring Vice-Presidential (Section H) address by Professor Lillien J. Martin was read before the American Psychological Association at Teachers' College, her subject being "Personality as Revealed by the Content of Images." For his Presidential address before the American

Folk-Lore Society, Dr. Robert H. Lowie chose "Oral Tradition and History."

A majority of the papers read were presented through the American Anthropological Association.

The list of authors and titles of papers presented follows:

AITKEN, ROBERT T. Personal Equation in Mythology.

BARBEAU, C. M. (*a*) Property and Transactions Among the Natives of British Columbia; (*b*) The Resources of the Canadian Folk-Lore. (Both by title.)

BEAN, ROBERT BENNETT. The Geographical Distribution of Certain Human Types.

BUELL, ROWENA. Some Composite Italic Charms (Exhibit of Collection).

DEARBORN, GEORGE VAN N. Blood Pressure and the Mental Process. (By title.)

FARABEE, W. C. A Brief Account of the University Museum's (Phila.) Expedition Among the Indian Tribes of the Amazon Valley.

HAGAR, STANSBURY. Significance of the Uxmal Temples.

HAWKES, E. W. The Argillite Culture of New Jersey.

—— Bering Strait as a Centre of Eskimo Migration and Dispersal.

HOUGH, WALTER. Exploration of a Pit Village near Luna, New Mexico. (By title.)

—— Fire Origin Myths of the American Indians. (By title.)

HRDLIČKA, A. Some Recent Finds of Supposedly Ancient Human Remains on this Continent.

JOHNSON, ANNE. La Gui-Année, a Missouri New Year's Custom.

JUDD, NEIL M. Evidences of Circular Kivas in Ruins of Western Utah.[1]

KUNZ, GEORGE F. Jade from Jordansmühl, Silesia, Germany.

LEDEN, CHRISTIAN. Notes on the Music of Greenland and Canada.

LEUBA, JAMES H. Differentiation of the Meaning of "Soul" and of "Ghost."

LOWRY, LAWSON G. (*a*) On Cranial Measurements of Persons Dying in Insane Hospitals; (*b*) On a Certain Anomaly of Gyration in Brains of Psychotic Persons. (Both by title.)

MACCURDY, GEORGE GRANT. Archeological Evidences Bearing on Man's Antiquity at Vero, Florida.

MEANS, PHILIP AINSWORTH. Translation and Annotation of an Interesting and Early Description of Palenque, written in 1786 by Juan Bautista Muños.

MERWIN, BRUCE W. The Obion Group of Mounds, Henry County, Tennessee.

MICHELSON, TRUMAN. Notes of the Peoria Indians.

MOOREHEAD, WARREN K. Andaste Cemetery on the Susquehanna.

MORRIS, EARL H. The Place of Coiled Ware in Southwestern Pottery.[2]

OETTEKING, BRUNO. The Processus Frontosphenoidalis of the Zygoma and its Bearing on the Configuration of the Orbit.

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[1] This article appears on page 34.

[2] This article appears on page 24.

PARSONS, ELSIE CLEWS. Abstract of Three Matrix Tales of Negro Folk-Tales.

—— Substitution of Animals for Men in Negro Folk-Tales.

PEABODY, CHARLES. (*a*) J. Reed Moir and the Rostro-Carinates; (*b*) A Prehistoric Wind Instrument from Pecos.[1]

POYNTER, C. W. M. Some Conclusions Based on Studies in Cerebral Anthropology.

SCHLEITER, F. Variability in the Implications of the Concept of Causality in Different Phases of Culture.

SOUTHARD E. E. and TAFT, A. E. The Comparative Convolutional Complexity of Left and Right Hemispheres.

—— The Comparative Convolutional Complexity of Subjects Drawn from Various Occupational Groups. (Both by title.)

SPECK, FRANK G. Kinship Terms and the Family Hunting Band.

—— Some Totems Among the Northeastern Algonkians.[2]

SPIER, LESLIE. Chronology of the Zuñi Ruins.

SPINDEN, H. J. Extension of the Archaic Horizon.

STAMP, HARLEY. (*a*) The Origin of Wampum, an Algonkin Tale; (*b*) Blood Pressure Experiments on American Indians.

SULLIVAN, LOUIS. Variations in the Glenoid Fossae.[3]

SWIFT, WALTER B. Some Psychological Aspects of Stuttering.

TAFT, A. E. and SOUTHARD E. E. The Comparative Convolutional Complexity of Left and Right Hemispheres.

—— The Comparative Convolutional Complexity of Subjects Drawn from Various Occupational Groups. (Both by title.)

THOMPSON, STITH. Bible Stories Among the American Indians.

WALSH, JAMES J. Humanity and Men. (By title.)

WARDLE, H. NEWELL. The So-called "Ceremonial Stones" in Relation to Algonkin Symbolism.

WILSON, LUCY L. W. Preliminary Report on Excavations at Otowi, New Mexico, by the Philadelphia Commercial Museum in 1915–16.

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[1] This article appears on page 30.

[2] This article appears on page 9.

[3] This article appears on page 19.

CONSTITUTION OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION[1]

ACT OF INCORPORATION

THE AMERICAN ANTHROPOLOGIC(AL) ASSOCIATION.	Recorded March 26, A.D. 1902. 10.20 A.M.

City of Washington,
District of Columbia.

We, the undersigned, Stewart Culin of Philadelphia, George A. Dorsey of Chicago, J. Walter Fewkes of Washington, W J McGee of Washington, and Joseph D. McGuire of Washington, being persons of full age and citizens of the United States, and a majority being citizens of the District of Columbia, pursuant to and in conformity with sections 599–604 of the code of law for the District of Columbia, enacted by the Senate and House of Representatives of the United States of America in Congress assembled and approved March 3, 1901, hereby associate ourselves together as a body corporate, and certify in writing:

1. That the name of the body corporate is The American Anthropologic(al) Association.

2. That the term for which the Association is organized is perpetual.

3. That the particular business and objects of the Association are, to promote the science of anthropology, to stimulate and coordinate the efforts of American anthropologists, to foster local and other societies devoted to anthropology, to serve as a bond of union among American anthropologists and anthropologic(al) organizations present and prospective, and to publish and encourage the publication of matter pertaining to anthropology; the Association to have power (*a*) to acquire, hold and convey real and other property and to establish general and special funds, (*b*) to hold meetings, (*c*) to conduct, support, or aid anthropologic(al) research, (*d*) to publish and distribute matter pertaining to anthropology, and (*e*) in general to transact any business pertaining to an organization for the promotion of anthropology.

4. That the affairs, funds, and property of the Association shall be in general charge of a board of managers whose number for the first

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[1] As amended at the New York Meeting, December, 1916.

year shall be thirty-two, all of whom shall be elected from among the members of the Association.

Witness our hands and seals this twenty-fourth day of March, 1902.

Stewart Culin (Seal)
George A. Dorsey (Seal)
Jno. D. McChesney J. Walter Fewkes (Seal)
(*Witness*) W J McGee (Seal)
Joseph D. McGuire (Seal)

Personally appeared before me this twenty-fourth day of March, 1902, Stewart Culin, George A. Dorsey, J. Walter Fewkes, W J McGee, and Joseph D. McGuire, all of whom signed the foregoing Act of Incorporation in my presence, and acknowledged the same in due form of law to be their act and deed for the purposes herein stated.

Done in the City of Washington, District of Columbia, this twenty-fourth day of March, 1902.

John D. McChesney,
Notary Public

(*Recorded on pages 33–34, book 10, Acts of Incorporation, Office of Recorder of Deeds, Washington, D. C.*)

Constitution (or By-Laws[1])

As Amended To December 29, 1916

Article I.—*Name*

The name of this body corporate is the American Anthropological Association.

Article II.—*Objects*

The objects of the Association are to promote the science of Anthropology; to stimulate the efforts of American anthropologists; to coordinate Anthropology with other sciences; to foster local and other societies devoted to Anthropology; to serve as a bond of union among American anthropologists and American anthropological organizations present and prospective; and to publish and encourage the publication of matter pertaining to Anthropology.

Article III.—*Membership*

Section 1. The Association may include four classes of membership, *viz.*, members, life members, honorary members, and patrons.

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[1] Under the laws of the District of Columbia an act of incorporation is equivalent to a constitution, and the more detailed regulations become by-laws.

SECTION 2. Persons or institutions interested in Anthropology may be elected on nomination of two members of the Association, and on payment of dues shall become Members of the corporation, with full rights of voting and holding office.

SECTION 3. Any person but not a library or other institution may become a Life Member on payment of $100 at one time.

SECTION 4. Persons who have obtained eminence through the promotion of Anthropology may be elected as Honorary Members; they shall be entitled to vote and hold office, shall receive the publications of the Association, and shall be exempt from dues.

SECTION 5. Persons interested in Anthropology who may at one time contribute $500 or more to the Association may be elected as Patrons; they shall be eligible as Members or Honorary Members, shall receive the publications of the Association, and shall be exempt from dues.

SECTION 6. All elections to membership shall be by the Council of the Association.

ARTICLE IV.—*Affiliations*

The Association may affiliate with other organizations.

ARTICLE V.—*Officers*

SECTION 1. The officers of the Association shall comprise a President, four Vice-Presidents, a Secretary, a Treasurer, an Editor, and a number of Councillors to be determined annually by the Association. These, with the ex-presidents, shall constitute a board of managers to be known as the Council.

SECTION 2. The President, Secretary, Treasurer, and Editor shall be elected annually to serve for one year, or until their successors are elected and installed. One Vice-President and one-fourth of the total number of Councillors shall be elected annually to serve for four years or until their successors are elected.

SECTION 3. The administration of the Association, including the filling of vacancies, the nomination of officers, and the arrangement of affiliations, shall be entrusted to the Council. Five shall constitute a quorum. The President, Secretary, Treasurer, Editor, and three additional members of the Council to be designated by the Council, shall form an Executive Committee of the Council, which shall meet at the call of the President and act in behalf of the Association, except during the meetings of the Association or of the Council, in all matters requiring attention.

SECTION 4. The President shall preside at the meetings of the

Association, of the Council, and of the Executive Committee, or may delegate this duty; the President and Secretary shall sign all written contracts and obligations authorized by the Council.

SECTION 5. In the absence or incapacity of the President his duties shall devolve on the Vice-Presidents in the order of their seniority in service.

SECTION 6. The Secretary shall record the proceedings of the Association, of the Council, and of the Executive Committee, conduct correspondence, make an annual report, and have general charge of executive matters under the direction of the President.

SECTION 7. The treasurer shall have charge of the funds and other property of the Association under regulations fixed by the Council, shall make collections and disbursements, and shall render an annual report; he may be required to give bond; and his accounts shall be audited annually and at such other times as the Council may direct, by a committee of three members of the Association.

SECTION 8. The Editor shall act as Secretary of the Committee on Publication and perform other duties as directed by the President, Executive Committee, or Council.

SECTION 9. The officers of the Association shall be elected by majority ballot of the members present at regular annual meetings after open nomination, including the nominations submitted by the Council.

ARTICLE VI.—*Meetings*

SECTION 1. A regular meeting of the Association and of the Council shall be held annually in December or January; this shall be the annual meeting for the election of officers and the transaction of general business.

SECTION 2. Special meetings of the Association or of the Council may be held on vote of the Council. Special meetings of the Council may be called by the President with the concurrence of two Vice-Presidents; and the President shall call a special meeting of the Council at any time and place on the written demand of ten members of the Council.

SECTION 3. Notices of regular meetings shall be published at least two months in advance, and printed notices of meetings, with preliminary programs of the scientific proceedings, shall be sent to all members at least a week in advance.

SECTION 4. Twenty members shall constitute a quorum of the Association.

ARTICLE VII.—*Committees*

SECTION 1. There shall be standing committees on Program, Finance, and Publication. These committees shall be appointed at the annual meeting by the incoming President, to serve until their successors are designated. The Committee on Publication shall form an editorial board in charge of the publications of the Association.

SECTION 2. All committees of the Association and of the Council shall be designated by the President, except when otherwise provided; and the President shall be chairman *ex officio* of every committee, except when otherwise provided.

ARTICLE VIII.—*Publications*

The Association may publish a periodical journal and occasional memoirs, and the periodical journal shall be sent to all members not in arrears, and to life members, honorary members, and patrons, and may be sold by subscription or otherwise.

ARTICLE IX.—*Finances and Property*

SECTION 1. The Association may acquire, hold, and convey property, both personal and real.

SECTION 2. The fiscal year of the Association (Society) shall begin on the first day of January.

SECTION 3. The annual dues of members shall be $6.00; for members of local affiliated societies that subscribe for the *American Anthropologist* for each of their members and which have adopted the *American Anthropologist* as their official organ, the annual dues shall be $1.00.

SECTION 4. Annual dues shall be payable in January; members one year in arrears shall not be entitled to vote or to receive the journal, and members two years in arrears shall, after formal notification, be regarded as withdrawn from the Association.

SECTION 5. The income from life membership and such other moneys as may from time to time become available shall be regarded as a permanent fund, which may be invested in the interests of the Association. The income from annual dues, patrons' payments, interest on investments, and other sources may be regarded as a working fund, available for publication and other current expenses; but any surplus of this fund beyond current needs shall be added to the permanent fund.

SECTION 6. No financial obligation in excess of available funds in the treasury shall be assumed by the Council except by authority of a two-thirds' vote of the members present at a regular annual meeting; *provided*, that for the purpose of this section annual dues and subscriptions for the current year may be reckoned as available funds.

ARTICLE X.—*Amendments*

SECTION 1. Amendments to this Constitution proposed by any three members in writing shall be referred to the Council, and notice thereof shall be given in the announcement of the next meeting; they shall be brought up at this meeting, and if approved by the Council may be adopted by a majority vote of the members present, but if disapproved by the Council shall not be adopted without a two-thirds' vote of the members present.

SECTION 2. Amendments shall go into effect immediately on adoption.

CONSTITUTION OF THE AMERICAN ETHNOLOGICAL SOCIETY

CERTIFICATE OF INCORPORATION OF AMERICAN ETHNOLOGICAL SOCIETY, INCORPORATED

WE, the undersigned, being all of the members of the Executive Committee of the American Ethnological Society, an unincorporated association, such committee exercising the functions commonly exercised by a Board of Directors, desiring to incorporate said unincorporated association pursuant to the provisions of Section 5 of the Membership Corporation Laws, do hereby certify as follows:

First: That we are all of full age; at least two-thirds of us are citizens of the United States; and at least one of us is a resident of the State of New York.

Second: The said unincorporated association was organized for the following purposes: to promote inquiries into the origin, progress and characteristics of the races of man; by publishing and distributing documents; by arranging scientific meetings and public lectures; and by other means adapted to the ends for which the said association was established.

Third: A regularly called meeting of the members of the said unincorporated association was duly held on the 28th day of February, 1916, at 8:15 o'clock in the evening, at the building of the American Museum of Natural History, in the Borough of Manhattan, City of New York.

Fourth: In accordance with the provisions of Section 5 of the Membership Corporation Law, a notice of the time and place of such meeting and of the intention of said unincorporated association to incorporate was given at least thirty days before such meeting, either personally or by mail, by serving the same upon each member of such association whose residence or post office address was known. Proof of service of such notice is hereto annexed and made a part hereof.

Fifth: Such meeting, by the unanimous vote of all the members of such Society present and voting, authorized the Executive Committee thereof to incorporate such unincorporated society or association for the same purposes for which it then existed, in accordance with the provisions of the Membership Corporation Law, with the corporate name of American Ethnological Society, unanimously adopted at said meeting, as more fully appears by the certificate of the chairman and secretary of said meeting, which is hereto annexed and made a part hereof.

Sixth: The particular objects for which said corporation is to be formed, are the same for which the said unincorporated association was established, to wit: To promote inquiries into the origin, progress and characteristics of the races of man; by publishing and distributing documents; by arranging scientific meetings and public lectures; and by other means adapted to the ends for which this corporation is organized.

Pursuant to Section 5 of the Membership Corporations Law the members of the said unincorporated association are to become members of the American Ethnological Society, Incorporated, and all of the property of the said unincorporated association, or held by any person or persons for its use and benefit, is to rest in and become the property of the said corporation, subject to the provisions of said Section.

The said corporation is to be formed for the purpose of incorporating the American Ethnological Society, an unincorporated association.

The said corporation is to do all other lawful acts and things necessary for and incidental to the accomplishment of the purposes hereinbefore enumerated.

Seventh: The name of said corporation shall be AMERICAN ETHNOLOGICAL SOCIETY, INCORPORATED

Eighth: The territory in which the operations of said corporation are to be principally conducted, is the Borough of Manhattan, in the City, County and State of New York.

Ninth: The principal office of said corporation shall be located in the Borough of Manhattan, in the City, County and State of New York.

Tenth: The number of directors of said corporation shall be nine.

Eleventh: The names and places of residence, or the post office address, of the persons who at the time of the said meeting of such unincorporated association constituted its Executive Committee, and who are to be the directors of said corporation until its first annual meeting are:

Names	*Residence or Post Office Address*
PLINY EARLE GODDARD,	Leonia, New Jersey.
ALFRED L. KROEBER,	University of California, Berkeley, California.
MARSHALL H. SAVILLE,	10 East 33rd Street, Manhattan, New York City.
ROBERT H. LOWIE,	448 St. Nicholas Avenue, Manhattan, New York City.

ELSIE CLEWS PARSONS,	115 East 72nd Street, Manhattan, New York City.
FRANZ BOAS,	Franklin Avenue, Grantwood, New Jersey.
CLARK WISSLER,	457 West 123rd Street, Manhattan, New York City.
A. A. GOLDENWEISER,	550 Riverside Drive, Manhattan, New York City.
STANSBURY HAGAR,	48 Wall Street, Manhattan, New York City.

Twelfth: The annual meeting of the said corporation shall be held on the last Monday in January in each and every year.

In Witness Whereof we have made, signed and acknowledged this certificate this 11th day of May, 1916.

(Signed by the above named persons.)

STATE OF NEW YORK
OFFICE OF THE SECRETARY OF STATE
ALBANY

It is hereby certified, that

The Certificate of Incorporation of American Ethnological Society, Incorporated, with acknowledgment thereto annexed, was filed and recorded in this office on the twenty-second day of May, 1916

Witness my hand and the seal of office of the Secretary of State, at the City of Albany, this 22nd day of May, 1916.

(Signed) A. B. PARKER, *Deputy Secretary of State*

AMERICAN ETHNOLOGICAL SOCIETY (INCORPORATED)

CONSTITUTION[1]

ARTICLE I.—*Name*

This corporation shall be named THE AMERICAN ETHNOLOGICAL SOCIETY (INCORPORATED).

ARTICLE II.—*Objects*

The object of the Society is to promote inquiries into the origin, progress, and characteristics of the races of man; by publishing and distributing documents, by arranging scientific meetings and public lectures, and by other means adapted to the ends for which the Society is established.

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[1] Adopted at a meeting of the Society held October 25, 1916.

ARTICLE III.—*Membership*

SECTION I. The Society shall consist of members (including annual members, life members, and patrons), fellows, corresponding members, and honorary members.

SECTION 2.—*Members.* The name and residence of a candidate for membership shall be presented in writing to the Board of Directors, and a majority vote of the directors present at any meeting of the Board of Directors shall be required for the nomination of the candidate.

Any person so nominated, and elected by the majority vote of the members present at any regular meeting of the Society shall become a member on the payment of the required dues.

SECTION 3.—*Fellows.* Any person engaged in work in anthropology or any related science may be elected a fellow in the manner provided in SECTION 2 for the election of members. Fellows shall have all the rights possessed by members of the Society.

SECTION 4.—*Corresponding Members.* Any person not a resident of New York City and vicinity, interested in the purposes of the Society, and capable ot sustaining relations to it of mutual advantage, may be elected a corresponding member in the manner provided in SECTION 2.

SECTION 5.—*Honorary Members.* Any person who has rendered distinguished services in the interests of anthropology may be elected an honorary member in the manner provided in SECTION 2.

SECTION 6.—*Withdrawal and Suspensions.* Any member or fellow shall cease to be a member or fellow upon the filing by him with the Secretary of a written withdrawal or resignation from membership. Any member or fellow twelve (12) months in arrears in the payment of dues may be suspended from membership by a majority vote of the Board of Directors, and may be expelled by a similar vote after due notice in writing.

ARTICLE IV.—*Dues*

SECTION I. The annual dues of members shall be ten dollars ($10.). The annual dues of fellows shall be six dollars ($6.).

SECTION 2. Any member who shall contribute at one time one hundred dollars ($100.) shall be known as a life member, and shall thereafter be exempt from annual dues. Any member who shall contribute at one time five hundred dollars ($500.) shall be known as a patron, and shall thereafter be exempt from the payment of annual dues.

Corresponding members shall be exempt from the payment of dues. Honorary members shall be entitled to all the privileges of the Society without the payment of dues.

SECTION 3. Members, fellows, and honorary members shall receive the publications of the Society and the "American Anthropologist" free, and shall become members of the American Anthropological Association, that organization consenting, without further payment of dues.

Corresponding members shall receive the publications of the Society free.

ARTICLE V.—*Officers*

SECTION 1. The officers of the Society shall be a President, two Vice-Presidents, a Secretary, a Treasurer, an Editor, and three Councillors. Collectively they shall constitute the Board of Directors. The Councillors and officers of the Society shall be chosen by and from its members and fellows whose names appear at the time of the election upon the roll kept by the Secretary as is provided in SECTION 4 of this Article.

SECTION 2.—*The President.* The president shall preside at all meetings of the Society, shall call meetings of the Society and of the Board of Directors, appoint all standing committees, and shall sign with the Secretary all contracts and other formal papers authorized by the Society.

SECTION 3.—*The Vice-Presidents.* Upon the death, or in the absence of the President, the Vice-Presidents in the order of their election shall discharge the duties of the President.

SECTION 4.—*The Secretary.* The Secretary shall keep the records of the meetings of the Society and minutes of the business transacted by the Board of Directors; he shall preserve a record of the membership of the Society properly classified, and shall furnish copies of the same to the Treasurer and Editor as may be required; he shall conduct the official correspondence of the Society, and with the President shall sign all contracts authorized by the Society; he shall have the custody of the records, books, and collections of the Society, so far as not otherwise ordered by a majority vote of the Society.

SECTION 5.—*The Treasurer.* The Treasurer shall receive and disburse all moneys of the society, and keep the same in a depository approved by the Board of Directors; he shall be the custodian of all legal and financial documents of the society, except those enumerated in SECTION 4 of this Article; he shall deposit or invest all dues received from life members and patrons as a permanent fund, in such manner as shall be approved by the Board of Directors; he shall keep separate accounts of any moneys received by him for special purposes, including the permanent fund, and is to sign all checks, drafts, and other orders for the payment of money on certification by the President, Secretary, or Editor; he shall present to the Board of Directors before the annual meeting a report of the receipts and expenditures during the preceding business year.

SECTION 6.—*The Editor.* The Editor shall have general charge of the publications of the Society under the direction of the Board of Directors.

SECTION 7.—*Compensation.* The several officers and directors of the Society shall serve without compensation for their services.

ARTICLE VI.—*Board of Directors*

The Board of Directors shall present at the annual meeting of the Society a report, verified by the President and Treasurer, or by a majority of the directors, showing the whole amount of real and personal property owned by it, where located, and where and how deposited or invested; the amount and nature of the property acquired during the year immediately preceding the date of the report, and the manner of the acquisition; the amounts applied, appropriated, or expended during the year immediately preceding such date, and the purposes, objects, or persons to or for which such applications, appropriations, or expenditures have been made; and the names and places of residence of the persons who have been admitted to membership in the Society during such year; which report shall be filed with the records of the Society and an abstract thereof entered in the minutes of the proceedings of the annual meetings. The Board of Directors shall also submit at the annual meeting a budget for the year immediately following the meeting for the approval of the Society, and shall be charged with the execution of the work provided in the approved budget. The Board of Directors shall also transact all business of the Society not otherwise provided for, and shall have power to fill vacancies in its own membership or in any offices until the next annual election.

ARTICLE VII.—*Meetings*

SECTION 1. Six regular meetings shall be held annually between the months of October and April inclusive, at such times and places as the Board of Directors may determine.

SECTION 2. The Board of Directors shall call a meeting of the Society upon the written request of fifteen members.

SECTION 3. The annual meeting shall be held on the last Monday in January in each and every year. At this meeting the reports of the

Board of Directors and of the officers shall be presented and passed upon, officers and councillors for the ensuing year shall be elected; a budget shall be adopted; and all other business which may be proper and lawful shall be transacted.

SECTION 4. Notices for special and other meetings shall be sent by mail at least three days prior to the time of holding the meeting, and a notice so sent shall be considered sufficient notification.

ARTICLE VIII.—*Quorum*

Nine members present at the annual or any other meeting of the Society shall constitute a quorum for the transaction of business. At any meeting of the Board of Directors five present shall constitute a quorum, and at any meeting of a committee a majority of such committee shall constitute a quorum.

ARTICLE IX.—*Amendments*

The Constitution and By-Laws may be repealed, amended, or added to by a vote of two-thirds of the members present at any meeting provided twenty days' notice in writing of such proposed amendment or amendments be given. A statement of the service of notice shall be filed with the Treasurer at or before the meeting.

BY-LAWS

ARTICLE I.—*Business Year*

The business year of the Society shall commence on January 1 and end on December 31.

ARTICLE II.—*Dues*

SECTION 1. Members whose dues are in arrears for more than two years shall be dropped from the roll unless the Board of Directors shall otherwise determine.

SECTION 2. "The American Anthropologist" is the official organ of the Society. Members or fellows receiving "The American Anthropologist" from other sources are entitled to such a reduction of the annual dues as may be determined from time to time by the Board of Directors.

ARTICLE III.—*Guests*

The Board of Directors shall invite as regular guests with the privilege to participate in all scientific work of the Society, persons temporarily residing in New York City and vicinity who are devoting themselves to anthropological researches and studies.

ARTICLE IV.—*Elections*

SECTION 1. At each annual meeting there shall be elected by ballot a President, two Vice-Presidents, a Secretary, a Treasurer, an Editor, and three Councilors, and as many additional officers and Councillors as may be required by the Constitution, to serve for one year or until their successors are chosen.

The officers of the American Ethnological Society, elected in January, 1916: President, PLINY E. GODDARD; First Vice-President, A. L. KROEBER; Second Vice-President, M. H. SAVILLE; Secretary, ROBERT H. LOWIE; Treasurer, ELSIE CLEWS PARSONS; Editor, FRANZ BOAS; Councillors, CLARK WISSLER, A. A. GOLDENWEISER, and STANSBURY HAGAR shall serve under this Constitution until the annual meeting in January, 1917, and until other officers are elected and have qualified.

SECTION 2. The Board of Directors shall serve as a nominating committee at the elections of the Society, but any other or additional nominations may be made by any member or fellow of the Society.

PROCEEDINGS OF THE AMERICAN ETHNOLOGICAL SOCIETY

Meeting of February 24, 1915

With President F. Boas in the chair, Dr. Gudmund Hatt delivered a lecture, illustrated with lantern slides, on "Beliefs and Customs of the Lapps." The following abstract was furnished by the lecturer:

Although the Lapps have long been under the influence of European civilization and are thoroughly christianized, many important survivals still exist in their customs and beliefs. They believe in different kinds of supernatural beings. Some of these are thought of as a people (sometimes called the *saivo*-people), living very much like the Lapps, but generally invisible and mostly underground. This underground-people seem originally to be the dead, although this idea is now obscured. Another class, but not distinctly divided from the aforementioned, is that of the sickness-bringing spirits, *muonas*, who still are clearly understood to be spirits of the dead. Different, but not always clearly distinguished by the Lapps from these spirits are the local spirits, residing in rocks, rivers, springs, mountain localities, etc. Important supernatural powers are connected with the lodge, where the fireplace and the place behind it are held especially sacred. The fire, and also certain springs have intimate connection with the renewal of life. Speaking of supernatural powers, the Lapps sometimes will emphasize the idea of spirits having definite personality; but at other times the idea of power itself is so highly emphasized that it seems devoid of personality. This comes out especially in magic, which is still practised, although the magic drum has disappeared. Characteristic of the Lapp mind is the idea of reindeer-luck, inherited or acquired by supernatural means, especially by offerings. The herding, milking, and slaughtering of the reindeer and dividing of the meat are regulated by a complex system of rules and traditions, which are all meant to insure reindeer-luck and reincarnation of reindeer, and which constitute a very essential part of Lapp beliefs and customs.

Meeting of March 5, 1915

The Executive Committee met. Present: Boas, Goldenweiser, Hagar, Goddard, Saville, Spinden, Mrs. Parsons.

A draft of the revised Constitution and By-Laws was read, discussed, and adopted for recommendation to the Society.

Dr. Goddard brought up the subject of an International Journal of American Linguistics and the possible connection with it of the Society. On motion of M. H. Saville it was decided to have the Chair appoint a committee of three to report on the matter to the Executive Committee.

Meeting of March 25, 1915

DR. H. J. SPINDEN delivered a lecture, illustrated with lantern slides, on "Culture History in Central America."

In Salvador there are abundant pottery remains of at least three cultures. The first is identical with archaic culture of the Valley of Mexico. The second is largely Mayan and the third is clearly Nahuan, corresponding to the late Toltec and to the Aztec periods in Mexico. The archaic culture is probably to be attributed to the Nahua tribes of the Mexican group who carried it as far as Nicaragua and Costa Rica. There is clear evidence of the transition from the archaic to the Mayan styles in Salvador and in the Totonacan region of Vera Cruz. In Nicaragua the Luna ware seems to be equivalent to the Mayan culture. In Chiriqui, the so-called alligator ware is apparently a late development of the archaic in which the plastic technique has been overcast by a graphic technique. In Nicoya there are many examples halfway in development between the archaic figurines of Salvador and the amulets representing nude females belonging to the alligator ware of Chiriqui.

The archaic culture is the most homogeneous and widespread and it may have risen immediately after the invention of agriculture. Maize is referred to the highlands of Mexico. Migration seems to have followed the zones of environment. The Nahua tribes seem to have preferred a high and dry country while a migration bringing in South American features seems to have followed up the humid east coast of Central America.

Meeting of April 28, 1915

AT this meeting Dr. Dean S. Fansler, of Columbia University, presented a paper on "Filipino Nursery Rhymes and Folk-Tales." He pointed out the several foreign influences—Hindu, Mohammedan, European—to which the Christianized Filipino have been subjected. He found that many tales could not be definitely traced to their source of origin for this reason, but some, especially the fairy tales, may be accepted as genuinely aboriginal. The lecture was discussed by Drs. Parsons, Spinden, Goldenweiser, and Lowie.

Special Meeting of May 20, 1915

A SPECIAL meeting having been called by the President, the Society met at the School of Journalism Building, Columbia University, at 4:30 p.m. Present: Boas, Goldenweiser, Hagar, Spinden, Spier, Blackie, Goddard, Mead, Mrs. Parsons, Lowie.

The draft of the revised Constitution, as passed by the Executive Committee, was read article by article and adopted as read. The By-Laws were also adopted article by article, with an amendment to Section 1, Art. IV. The Constitution and By-Laws were then adopted as a whole.

Dr. Goddard moved, and it was passed, that the Chair appoint a Committee of three to take up the matter of incorporating the Society.

Mr. Hagar announced the resignation of Mr. Streeter and Mrs. Guy. The meeting then adjourned.

A meeting of the Executive Committee was then called by the President. Mrs. Parsons proposed for membership Mr. Charles Sheldon, and the Committee unanimously indorsed the nomination.

Meeting of October 25, 1915

THE Society met in conjunction with the Section of Anthropology and Psychology of the New York Academy of Sciences, with Dr. Clark Wissler in the chair. The following public lecture was delivered: Marshall H. Saville, "Field Activities of the Museum of the American Indian, 1915."

Professor Saville outlined the operations carried on under the auspices of Mr. George G. Heye during the past year. The work achieved was both archaeological and ethnological, and embraced South America as well as North America. Somatological material is not to be installed by the Museum, but is turned over to the National Museum in Washington, D. C. The speaker summarized the investigations carried on by himself, Mr. Theodor de Booy, and others, illustrating his lecture with slides.

Meeting of November 29, 1915

THE meeting was preceded by a session of the Executive Committee, attended by Boas, Hagar, Goddard, Lowie. Mr. Paul Kelly was proposed for membership, and Miss Bella Weitzner, Dr. H. K. Haeberlin, and Mr. Robert G. Aitken were proposed for fellowship in the Society. The Committee unanimously voted to nominate the above-mentioned.

The meeting of the Society was then called to order by Professor Franz Boas, with a few words of greeting to Professor Alfred L. Kroeber, of the University of California, who will spend the winter in New York and is expected to participate in the meetings of the Society. The member and fellows nominated by the Executive Committee were elected.

Mr. Leslie Spier presented a paper, illustrated with charts, on "Archaeological Research at Trenton, N. J.," as pursued by the Department of Anthropology of the American Museum of Natural History. A survey of the archaeology of the Coastal Algonkin region necessitated an investigation of Abbott's "argillite culture," represented in the yellow sand below the surface soil at Trenton. Implements found in quantities differed culturally from the remains of the historic Lenapé, only a few types of blades, arrow points, and hammerstones being present. As shown by tabulated records, these artifacts occur in small groups. These appear over an extensive area, lying in a single plain of deposition near the surface soil. The vertical distribution of artifacts in these groups is uniform throughout the area, approximating a normal frequency distribution with a vertical range of dispersion of about twelve inches.

Having defined the typical manner of distribution of these implements, their relations to certain geological features were investigated. The position of the horizontal warped planes (the so-called "red bands") which traverse the yellow sand, bear only a casual relation to the distribution of the artifacts, and may be considered as of secondary formation. The vertical distribution of the pebbles in the yellow sand also approximates a normal frequency distribution. The identity of the positions of the norms, and the range and manner of dispersion of corresponding series indicate the intimate relation which exists between pebble and artifact distributions. So far as the Trenton deposit is concerned, the problem is thus reduced to the determination of the geological antecedents of the several members comprising the stratum.

Mr. Spier's paper was discussed by Mr. Linton, who had done archaeological work in other parts of New Jersey, and by Dr. Chester A. Reeds, who participated as geologist in the expedition reported on by Mr. Spier. Dr. Clark Wissler pointed out as a distinctive feature of the new Trenton operations the use of statistical methods.

Dr. Robert H. Lowie then presented "Field Notes among the Kiowa and Hopi." Among the Kiowa his attention had been confined to the military societies, which were six in number, only one of which, the Rabbits, could in any sense be described as an age-division. Moreover, the element of purchase was lacking so that altogether the Kiowa system suggests that of the Crow and Cheyenne rather than of the Arapaho or Mandan. The points that required investigation among the Hopi relate to social organization, more particularly to the kinship nomenclature. This differs from the Plateau Shoshonean type in the absence of reciprocal terms and also in the merging of collateral and lineal blood-relatives, the latter feature corroborating Rivers' correlation of the "classificatory" kinship system with a clan grouping. Certain specific features characterize the Hopi system. There is a merging of distinct generations analogous to that which occurs among the Crow and Hidatsa, and which is explicable through the influence of the clan. Then there is a markedly weak development of terms of affinity, with teknonymy fairly prominent. Finally, there is a certain looseness in the use of kinship terms as compared with the rigor found elsewhere. Certain of these features seem to be common to all the Pueblo Indians. The clan of the First Mesa corresponds in constitution to the Iroquois maternal family since a census of all the clans shows that each consists of an old woman with her brothers and sisters, lineal and collateral, and her own and her lineal and collateral sisters' descendants through females. It will, of course, be necessary to correlate this information with data from the Second and Third Mesas. An institution found among the Navajo but lacking among the Hopi is the mother-in-law taboo, the precise distribution of which in North America should be determined.

There was an animated discussion in which Dr. Herbert J. Spinden, Professor A. L. Kroeber, Dr. A. A. Goldenweiser, Dr. Pliny E. Goddard, and Professor F. Boas took part.

Meeting of January 21, 1916

THE Executive Committee met at 2:15 p.m. Present: Boas, Goddard, Hagar, Lowie, Mrs. Parsons.

The minutes of the last meeting were adopted as read. Dr. Goddard presented the report of the Incorporation Committee to the effect that the matter of incorporation had been entrusted to a proper representative who advised that the Constitution and By-Laws be revised to conform to legal requirements. Dr. Goddard further expounded the situation that had arisen in connection with the *American Anthropologist* and the American Anthropological Association. It was moved by Hagar, seconded by Lowie, and passed for recommendation to the Society that hereafter the dues of Fellows be raised to $6.00, the extra dollar to be paid to the Association and to make them automatically members of that organization, to meet the expenses incident to the publication of the *Anthropologist*. It was further passed that hereafter this journal be furnished only to members and fellows. Mr. Hagar moved and it was passed that the Committee recommend that all life-members and members be made members of the Association in the same manner as the fellows except in so far as they may already be members. The meeting then adjourned.

Meeting of January 24, 1916

The Executive Committee met before the annual meeting of the Society. Present: Boas, Goddard, Lowie, Mrs. Parsons. Dr. Goddard reported for the Committee on the legal changes necessary in the Constitution before incorporation. It was moved and carried that notice of these amendments was to be given at the meeting of the Society.

The Secretary presented the report transmitted by the Treasurer, which was referred to an Auditing Committee composed of Messrs. H. S. Stiles (chairman), H. K. Haeberlin and N. C. Nelson.

The following were nominated for office: President, P. E. Goddard; 1st Vice-President, A. L. Kroeber; 2d Vice-President, M. H. Saville; Secretary and Librarian, R. H. Lowie; Treasurer, Elsie Clews Parsons; Editor, Franz Boas; Executive Committee (in addition to the above), A. A. Goldenweiser, Clark Wissler, Stansbury Hagar. The meeting then adjourned, to be followed by the annual meeting of the Society.

President Franz Boas called upon the Committee of Incorporation to give notice of the proposed amendments to the Constitution and By-Laws rendered necessary by legal requirements. This was done by Dr. Goddard.

The President next called on the Secretary to present his report. The Secretary reported that the Society had the following membership: Life Members, 13; Subscribers, 12; Anthropological Fellows, 22; Fellows, 53; Members, 13; Non-Resident Members, 3. The Secretary's report was adopted.

The Treasurer's report was then read by the Secretary and referred to the Auditing Committee appointed by the Executive Committee (for the report of the Auditing Committee, see below).

The Editor presented his report on publications, which was adopted.

The Secretary, acting for the Nomination Committee, presented their nominations for officers for the year 1916. On motion the Secretary was instructed to cast a single ballot for the nominees, as listed above.

With this the business meeting closed and Professor Franz Boas presented some "General Ethnological Notes from Porto Rico." He was followed by Mr. Robert G. Aitken, who delivered a lecture, illustrated with slides, on "Porto Rican Burial Caves." This address embodied a brief preliminary report to the Society on archaeological work done during the past summer in Porto Rico, under the auspices of the New York Academy of Sciences and the Porto Rico Insular Government. This work was in two sections, the first being the excavation of a cave and adjacent village site, the second the excavation of a second and much larger site. This paper is concerned only with the first section of the work. The work was performed by Dr. J. Alden Mason and the writer, under the general direction of Dr. Franz Boas. The actual time spent was about two weeks, during the latter part of June, and the early part of July, 1915.

The cave in question was one of the innumerable hollows in the limestone formation which composes a large portion of the Island. Many of these caves are difficult of approach, or even practically inaccessible, but this one fortunately was quite easily reached and excavated. It lies in a ridge about ten miles from the city of Utuado, is at an elevation of about two hundred feet above the floor of the adjacent valley, and is less than a mile from a well traveled trail. The entrance is large and faces east, giving plenty of light and making the floor quite dry.

The floor of the cave was entirely excavated, a layer at a time. It was composed of alternate layers of disintegrated limestone and crystalline calcite, with a thick underlying stratum of clay. The upper forty inches of the stratified formation, which was at most five feet thick, yielded no less than twenty fairly complete human skeletons, all evidently having been interred. The method of burial was fairly uniform, the bodies being in a contracted position, the knees drawn up in front of the body and the arms doubled up similarly. Most of the burials were near the north wall of the cave, the bodies evidently having been placed on their sides, facing the wall. All the remains were quite fragile, but hardened on exposure to the air. Only three complete intact skulls were found, but it is possible that some few of the others may be so nearly complete as to admit of restoration.

No evidence was found of mortuary offerings with the bodies. In fact, with the exception of a few bits of stone and numerous snail and crawfish shells, there was nothing found but the bones. A few scattered bits of potsherd were found, but not enough to even guess at the shape of the vessels of which they were parts. All were of a dull red-brown, unglazed ware.

The village site at the foot of the hill yielded nothing save a few more bits of similar potsherds, a single hammerstone, and a few pebbles.

The fact that it was a village site was proved by the presence of prehistoric walls marking off small terraces and courts of irregular shape. Two small mounds yielded nothing.

The specimens collected were all shipped to the American Museum of Natural History in New York, where they await arrangement and study. The deductions we may at present draw from the material collected are: that the Porto Ricans did practise burial in caves; that these caves were not used as residences when used as burial grounds; that these caves were associated with village sites at no great distance; that mortuary offerings were not placed with the dead.

Further investigation of the many other caves may strengthen these deductions, and will establish as proven, many facts that we, on the basis of our scanty material, may hardly suggest as possibilities.

Another report on "Archaeological Work in Porto Rico" was then presented by Dr. H. K. Haeberlin. In June, 1915, the lecturer undertook some archaeological research in the limestone mountains near Utuado, Porto Rico. A so-called "ball-court" was studied. It lay in the bottom of a valley and consisted in a rectangular enclosure 50 ft. long and 30 ft. wide. On the north and the south the court was bordered by a continuous row of flat stones, which varied from one foot to over three feet in length. On the east and west sides there were no stones. The original floor of the enclosure was 6 inches deeper than the surrounding country. Red pottery with incised scroll-work was found.

A cave was also excavated. Here enormous quantities of snail shells, bones of rodents, and crab bones were found together with many potsherds. The latter represents a type of pottery distinct from that found in the "ball-court." It is distinct in texture, shows no incised work, and is characterized by wide handles. A burial of a baby was unearthed. In a niche a bone pendant representing a face was found. Several stalactites showed rock-carvings in the form of faces.

The meeting then adjourned.

The cards sent to members announcing this meeting contained notice that at the meeting of February 28, 1916, a vote would be taken on the question whether the Society should incorporate.

Meeting of February 28, 1916

THE meeting was preceded by a session of the Executive Committee, with P. E. Goddard, A. L. Kroeber, F. Boas, and R. H. Lowie present. The motion was made to nominate Mr. Pierre A. Bernard for annual membership in the Society. Passed. The Secretary read the report of the Auditing Committee, which was accepted for submittal to the Society. The Executive Committee then formally approved the Guaranty Trust Company as a depository of the Society funds. The session of the Committee was then adjourned.

The meeting of the Society was called to order with President P. E. Goddard in the chair. The following members were present: Franz Boas, R. H. Lowie, L. Spier, N. C. Nelson, H. K. Haeberlin, B. Weitzner, H. S. Stiles, C. W. Mead, Wm. R. Blackie, P. E. Goddard, A. A. Goldenweiser.

The Secretary submitted the nomination of Mr. Pierre A. Bernard by the Executive Committee, and Mr. Bernard was unanimously elected a Member. The Secretary was then instructed to read the Report of the Auditing Committee, which follows:

REPORT OF THE AUDITING COMMITTEE, FEBRUARY 16, 1916.

Balance on hand January 19, 1915		$2,961.63
Received from members	$ 427.12	
Received from interest January 22, 1915	163.35	
Received from interest January 24, 1916	78.16	668.63
		$3,630.26

Expenditures.

Publications including *American Anthropologist* vol. 17, nos. 1, 2, 3	903.95	
Stationery	26.45	
Treasurer's cash account	13.30	
Secretarial and editorial work	12.00	
Miscellaneous refund	40.00	
Operator	9.00	1,004.70
Total balance January 31, 1916		2,625.56
On deposit with Metropolitan Trust Company	10.00	
On deposit with the Manhattan Savings Institute	2,214.60	
In the custody of Mrs. Elsie Clews Parsons	400.96	2,625.56

The expenditures exceeded the receipts as follows:

Total expenditures		1,004.70
Total receipts from members	427.12	
Total receipts from interest	241.51	668.63
Excess of expenditures over receipts		$ 336.07

N. C. NELSON,
HERMAN K. HAEBERLIN,
H. S. STILES, *Chairman.*

The Secretary reported that at least thirty days before this meeting he had mailed to each member of this association, a notice of the intention on the part of this association to incorporate under the New York Corporation Laws. The Secretary further reported that said notices were mailed to the last known residence or post-office address contained in the records of this association.

On motion duly made and seconded, it was unanimously resolved: That this association incorporate for the same purposes for which it is now in existence; that the executive committee be and it hereby is authorized to incorporate for said purposes, under the Membership Corporations Law of the State of New York, and to file a Certificate of Incorporation as provided in said law and to do all other lawful acts and things incidental to and necessary for the accomplishment of the purpose of incorporating this association.

On motion duly made and seconded it was unanimously resolved that the name of the American Ethnological Society shall stand.

On motion of A. L. Kroeber it was resolved to lay the amendments of the Constitution on the table for the next session.

The President now called on Professor A. L. Kroeber to present the paper of the evening on "The Zuñi Clan." (See *Anthropological Papers, American Museum of Natural History*, vol. XVIII, pt. II.)

Meeting of March 27, 1916

The Executive Committee met at 5 p.m. Present: Goddard, Boas, Kroeber, Lowie, Mrs. Parsons.

The matter of allowing for the exchange increment in favor of Dutch money, as demanded by the Dutch publishers of the Society, was considered. It was decided to determine through the treasurer what the exchange rate was at the time the remittance was sent, and that the matter be left in the hands of the Editor with power to pay half the difference.

The demand of a 25 percent increase of pay for future work, suggested by the printer, was next considered. It was moved by A. L. Kroeber that an increase of 12½ percent be allowed for the duration of the war with an adjustment to be made thereafter; this arrangement to apply to new matter but not to what had already been printed. An amendment was suggested by Mrs. Parsons to the effect that the increase be allowed for matter not in type, but not for matter already in print, with a readjustment to be made after the war. The amendment was accepted by A. L. Kroeber and passed.

Dr. Goddard reported on the delay in the incorporation proceedings.

The treasurer presented her report.

At 8:15 the Society held a meeting in conjunction with the Section of Anthropology and Psychology of the New York Academy of Sciences. Dr. A. A. Goldenweiser presented a paper entitled "Notes on Melanesian Social Organization," in which Dr. W. H. R. Rivers' recent *History of Melanesian Society* was subjected to a critical analysis.

Meeting of Monday, April 24, 1916

PRESIDENT P. E. GODDARD in the chair. The Secretary reported that the Executive Committee recommended for election Mr. M. D. C. Crawford as member and Mr. William Churchill as fellow of the Society. The Society ratified these nominations.

Mr. M. D. C. Crawford then delivered the lecture of the evening on "Cotton Spinning and Textiles of Peru." Mr. Crawford discussed this subject primarily from the point of view of a textile expert. He pointed out the antiquity of the art of weaving in Peru, its high development, and the fact that some of its products in cotton and wool remain unexcelled and unequaled by our modern looms. By way of illustration Mr. Crawford exhibited a series of specimens furnished for this purpose by the Department of Anthropology of the American Museum of Natural History. The paper was discussed by Mr. N. C. Nelson and there followed an informal demonstration of points by reference to the specimens.

Meeting of the Executive Committee, May 11, 1916

THE meeting was held at 4 p.m. Present: Goddard, Boas, Wissler, Saville, Hagar, Mrs. Parsons, Goldenweiser, Kroeber, Lowie.

The members conformed to the preliminary legal requirements of incorporation by signing statements prepared by a notary public who was present for the purpose.

The Secretary was instructed to canvass the State Universities and libraries of the country with a view to making them subscribers.

Meeting of October 23, 1916

THE meeting was held at 8:15 p.m. President P. E. Goddard was in the chair. The election of Mrs. Donohugh and Miss Marjorie A. Mallory as fellows was ratified by the Society. This being the first meeting of the incorporated Society, the articles of incorporation were read by the Secretary. The President then read the amendments to the Constitution, which were adopted by the Society. On motion of

Dr. F. Boas, the Constitution was then adopted as a whole by the American Ethnological Society.

Dr. P. E. Goddard then delivered a lecture on "The Social Organization of the Arizona Apache," with Dr. A. A. Goldenweiser in the chair. He discussed the local bands and more particularly the social divisions of the White Mountain Apache, who are grouped into four exogamous matrilineal units with numerous subdivisions. Special stress was laid on the functional aspect of these clans and interesting social customs, such as the joking relationship obtaining between certain kinsmen, the mother-in-law and the sister taboo, were described. With the aid of diagrams Dr. Goddard also gave an exposition of the Apache nomenclature for relatives by blood and marriage.

The paper was discussed by Drs. Boas, Goldenweiser, and Lowie.

The Chairman then called upon Dr. Lowie to present a paper on "The Hopi Clan." Dr. Lowie dealt briefly with the relations of the clan to the maternal family, the phenomenon of linked clans, the descent of ceremonial offices, household groups, and the kinship terminology.

The meeting adjourned at 10:30.

Meeting of November 27, 1916

THE Board of Directors met at 8:00 p.m. Present: Goddard, Boas, Wissler, Goldenweiser, Lowie. Dr. Boas asked for authority to proceed with the printing of the second part of Wm. Jones' texts. On motion of Dr. Wissler, this was unanimously granted. Dr. Boas then asked for permission to proceed with the publication of Swanton's *Haida Texts*. On motion of Dr. Wissler the matter was referred to the President and Editor with power to act. The Secretary presented the name of Mr. Earl H. Morris for fellowship in the Society, and he was unanimously nominated by the Board. The meeting was then adjourned.

The meeting of the Society was called to order by President P. E. Goddard. The Secretary presented Mr. Morris' name to the Society, and he was duly elected a fellow. The President then called upon Professor Franz Boas to deliver his address on "New Evidence on the Instability of Human Types." Professor Boas outlined the nature of the general problem of the relations of heredity and environment. The paper was discussed by Dr. M. Fishberg and a number of visitors. Meeting adjourned at 10 p.m.

Meeting of January 29, 1917

THE Board of Directors met at 8:00 p.m. The Secretary's and Treasurer's reports, with the statement of the auditing committee, were adopted by the Board for presentation to the Society. The Board also adopted the Budget report presented by the Secretary and unanimously nominated for fellowship Miss H. H. Roberts, Mr. Ralph W. Linton, Mr. Louis R. Sullivan, and Mr. Frederick Schleiter; and for membership Dr. J. L. Williams, Sr., 220 West 42d Street, New York City. As a nominating committee the Board accepted the following slate: President, P. E. Goddard; 1st Vice-President, Clark Wissler; 2d Vice-President, Marshall H. Saville; Secretary, Robert H. Lowie; Treasurer, Elsie Clews Parsons; Editor, Franz Boas; Board of Directors (in addition to the above), A. A. Goldenweiser, Stansbury Hagar, N. C. Nelson. The Board authorized the Treasurer to keep the Society's funds in the Guaranty Trust Co. and Manhattan Savings Institution, respectively. The meeting was then adjourned.

The annual meeting of the Society, held in conjunction with the Section of Anthropology and Psychology of the New York Academy of Sciences, was called to order at 8:20, with President P. E. Goddard in the chair. Members present: Boas, Mrs. Donohugh, Goddard, Hagar, Linton, Lowie, Morris, Nelson, Miss Roberts, Sullivan, Spinden, Spier, Miss Weitzner, Wissler. Among the visitors present were Dr. R. Porfirio Aguirre of Mexico and Mr. S. G. Morley.

The President called upon the Secretary to present his and the Treasurer's report with that of the auditing committee. The Secretary thereupon presented the following:

SECRETARY'S REPORT

The present membership of the American Ethnological Society, Inc., divided into classes, is as follows:

Life Members	14
Members	12
Fellows	61
Anthropological Fellows	22
Total	109

This includes the member and fellows elected at the present meeting. For comparison with earlier lists should be added the number of subscribers; the Secretary has record of 12, but this is incomplete since the number has been enlarged by our agent, G. E. Stechert and Company. This gives a minimum grand total of 121 as compared with 116 in January, 1915 and in January, 1916. The Society lost 2 Members by death, Mr. August Zinsser and Dr. M. Langmann, and one by resignation, Miss Myra B. Martin.

The Society held the usual number of meetings at which the following papers were presented:

(January) Franz Boas, General Ethnological Notes from Porto Rico.
Robert G. Aitken, Porto Rican Burial Caves.
H. K. Haeberlin, Archaeological Work in Porto Rico.
(February) A. L. Kroeber, The Zuñi Clan.
(March) A. A. Goldenweiser, Notes on Melanesian Social Organization.
(April) M. D. C. Crawford, Cotton Spinning and Textiles of Peru.
(October) P. E. Goddard, The Social Organization of the Arizona Apache.
R. H. Lowie, The Hopi Clan.
(November) Franz Boas, New Evidence on the Instability of Human Types.

On February 28, 1916 at a regular meeting of the Society a motion to incorporate was carried; on May 11, the Executive Committee conformed to the legal requirements by signing statements prepared by a notary public; on May 22, the Certificate of Incorporation was filed and recorded in the office of the Secretary of State.

Respectfully submitted,
Robert H. Lowie,
Secretary

Treasurer's Report

Receipts

Feb. 5, 1916	From S. Hagar and deposited in the Guaranty Trust Company	$400.96	
Mch. 24, 1916	From S. Hagar (not included in above)	10.00	
July 28, 1916	From Guaranty Trust Co., interest	3.70	
Dec. 28, 1916	From Guaranty Trust Co., interest	7.48	
	From Life Membership Paul H. Kelly	100.00	
	From dues collected to date	514.00	
	From collection charge on check	.10	
			$1,036.24

Disbursements

For printing and postage	$ 34.10	
For publications American Anthropological Association	241.46	
For collection charges on checks	.80	
For expenses of incorporation, American Ethnological Society	20.00	
For draft E. J. Brill of Leyden, Holland	13.58	
Balance	726.30	
		$1,036.24

January 1, 1917 Balance on Deposit in Guaranty Trust Co......$726.30

FUNDS OF THE AMERICAN ETHNOLOGICAL SOCIETY
ON DEPOSIT WITH
MANHATTAN SAVINGS INSTITUTION

Amount on Deposit February 1, 1916 (as per Bank Book No. 162136, received by Elsie Clews Parsons, Treasurer, from Stansbury Hagar)	$2,214.60
Interest to January 1917	78.16
Balance on Deposit	$2,292.76

Respectfully submitted
ELSIE CLEWS PARSONS,
Treasurer

NEW YORK CITY,
January, 1917

AUDITING COMMITTEE'S STATEMENT

The accounts of the Treasurer of the American Ethnological Society have been examined and found to be correct.

$100 for the life membership of Paul H. Kelly should be drawn from the year's funds and deposited in the Manhattan Savings Institution, as has been the custom with dues from life members. Only $90, however, of the total sum of $100 was paid this year. The initial $10 was paid a year ago, and does not appear on this year's accounts.

Respectfully,
LESLIE SPIER,
MARJORIE MALLORY,
Auditing Committee

NEW YORK CITY,
January 29, 1917

These reports were accepted as read. The President next called on the Secretary to present the Budget prepared by the Board of Directors. The following Budget was then read and accepted by the Society:

BUDGET

The available expenditure funds of the Society will be:

Interest from Permanent Fund	$ 78
Balance reported by Treasurer	626
Guaranteed by Carnegie Institution for publication of Jones material	900
Estimated income from dues	440
Total	$2,044

On this basis the Board of Directors submits the following Budget:

For the *Anthropologist*	$ 250
Printing notices and sundry expenses	40
Koryak volume	150
Jones, vol. I.	550
Jones, vol. II	627
Philological Journal	50
Swanton material	110
For membership in Anthropological Association	125
Total	$1,902

The President then asked the Secretary to present the Board's nominations for membership and the names favorably acted upon by the Board were read and elected members and fellows, respectively.

The President next called on the Secretary to read the nominations of officers for 1917. In the absence of any nominations from the floor, all the officers proposed by the Board were elected by acclamation. The business meeting thus closed and the President asked Mr. N. C. Nelson to present his paper on "The Southwest Problem."

Mr. Nelson said in abstract:

The Southwest as a field for investigating the course of development of elementary human activities has long been attractive. The reason for this is threefold: in the first place, the cultural phenomenon—that of the Pueblos—is of a relatively high order; in the second place, it is amply represented by prehistoric remains; and in the third place, it is a culture which is still subject to observation and which has been subject to observation for nearly four hundred years. It is a field, therefore, in which the historian, the ethnologist and the archaeologist may work together, their independent results being mutually supplementary.

Investigations of a preliminary nature began as long ago as 1846,—the year in which America took possession of the country—and have been gathering headway until today there are no less than nine public institutions in the field. The work has been prosecuted on all three lines—historical, ethnological and archaeological. The historical investigation is already well in hand; the ethnological work has made considerable progress; and the archaeological work—if lagging—is well under way. The territory to be covered is large and the problems presented, while not difficult, require time. At present the entire Pueblo range has been at least superficially examined and we possess extensive collections from nearly every one of the several separate centers in which the culture reached a high state of development.

The problem now before the archaeologist is a chronological one. He needs to know what has taken place in each specific center in order that, by the correlation of his facts, he may be enabled to determine the history of the Pueblos as a group—their origin, racially and culturally, as well as their antiquity. In this new phase of the investigation the American Museum has made a good beginning during the last four years, having already worked out the stratigraphy of two culture centers besides being in possession of most of the necessary data for two additional localities of primary importance.

The paper was discussed by Drs. Goddard and Spinden, Mr. Morley and Mr. Spier.

The meeting adjourned at 9:30 p.m.

ROBERT H. LOWIE,
Secretary

DISCUSSION AND CORRESPONDENCE

MISCONCEPTIONS CONCERNING DÉNÉ MORPHOLOGY—REMARKS ON DR. SAPIR'S WOULD-BE CORRIGENDA

THE fourth number of the *American Anthropologist* for 1915 contained a series of remarks by Dr. Sapir on my review of his *Notes on the Chasta Costa Phonology and Morphology* under a caption[1] to which exception could well be taken. It is not usual to see *errata* or *corrigenda* thus glaringly paraded by another than the party responsible for the production in which they are supposed to be found. In the present case such a title is a misnomer, and, even if warranted by the circumstances, it would come with a better grace from an old hand in the field of Déné linguistics. Most of the eleven statements which Dr. Sapir seems to criticize were simply proffered in a tentative way, as mere suggestions or bids for explanations.[2] One "corrects" assertions, not suggestions. Practically all of the few that were unequivocal affirmations are the expression of real facts, and therefore not any more *corrigenda* than the others. One exception refers to my taking Dr. Sapir's *A* for my *æ*, an error which is perhaps imputable to the printer, and should not certainly take eight pages of small text to "correct".[3] Moreover, with regard to two of his would-be *corrigenda* (Nos. 6 and 11), my critic admits that I was right.

I trust that, even in his kind reference to what he is pleased to call my "admirable mastery of the Carrier language," one is not to see something akin to a hint that I should confine myself to its exposition, a suggestion that has already appeared in print. To ward off the accusation of unbecoming presumption in treating of other related dialects, I am forced to remark, once for all, that a man who used to preach without an interpreter in Chilcotin before he knew one word of Carrier or Babine, and had acquired these two last idioms before he understood Sékanais, or who could catechize unaided in Nah'ane, five Déné dialects differing widely in their lexicon and grammar (with the exception of Carrier and Babine), that man, I say, should be able to detect errors or misapprehensions concerning phonetic and morphological points which run through the whole Déné linguistic family, especially when he has been, since 1882, studying those idioms, twenty-four years among those who speak them, and then with the help of all the literature bearing on the subject. It must be admitted that this help is not always infallible, because, occasionally, of an imperfect ear on the part of the transcriber, distractions or printer's errors, such as those which Dr. Sapir himself points out at the end of his remarks under Nos. 3 and 11.

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[1] *Corrigenda to Father Morice's "Chasta Costa and the Déné Languages of the North,"* vol. 17, p. 765.

[2] "I more than suspect;" "might not Dr. Sapir's informant have misunderstood the questioner?" "unless I am very much mistaken," etc.

[3] Dr. Sapir is very obscure with regard to the definition of the value of his graphic signs. Nowhere can I find an explicit statement concerning the phonetic identity of his A; the nearest approach to it is an incidental remark (p. 278): "*æ* (*probably* identical with our A) in Carrier," he says.

This scholar claims that Chasta Costa *tc!ᴀc* "can have nothing to do with this *tθ'aθ*, but must go back to Ath.[1] $^{*}k^{y}$*!ac*," and he props up his contention by remarking that, according to Dr. Goddard, *kīyauw* happens to have in Hupa the same signification, "birds," as his own *tc'ᴀc*. He likewise takes exception to my practically assimilating the suffix *-tc!e* of *li-tc!e* with that of my *li-tṣe*, declaring at the same time that this really corresponds to Ath. k^{y}*!e*, which assertion he similarly bases on the *-ke* of Hupa *liñ-kc*.

He might not be far astray if the guttural sound was accompanied by the glottal explosion which we find in C.C.[2] *tc!ᴀc*, Carrier and Montagnais *tṣ̌œš* (according to my orthography), Navajo *tṣos* (do.), Loucheux *tṣ̌aw* (do.), as well as in the various equivalents of "woman" (*tṣ̌e* or *tc!e*), an all-important explosion, or click, which Dr. Sapir renders by an exclamation point and I by a dot under the letter affected thereby. Dr. Sapir is himself so well aware of this that he obligingly supplies that "click" (p. 766) to the *ki-* of the first word and the *-ke* of the second! If Dr. Goddard really meant *ḳ* (Sapir's *k!*) when he wrote *k*, may I ask how, in that case, he rendered the common *k* sound?[3]

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[1] Athapascan.

[2] Chasta Costa.

[3] Years ago I criticized that scholar for having failed, as I thought, to render in his Hupa Texts the particular aspirated *t* (my *th*, the Franciscan Fathers' *tq*) common to all the Déné dialects. As a matter of fact, his *t* was the equivalent of my *th*, and he rendered the ordinary *t* by *d*. But I still fail to see how he expressed the real *d* sound, which does exist in Déné, though the natives themselves do not differentiate it from *t*. A. G. M.

The aspirated *k*, sound Father Morice writes ᴋ, becomes everywhere in Hupa a continuant written *x*. The character *k* was therefore used for the sound Father Morice calls "click," and writes *ḳ*. What Father Morice calls the common *k* is

As to the Hupa root *-tsōts* or *-tsōs*, which my critic adduces in this connection (Carrier, Chilcotin, Sékanais and Nah'ane *-tṣus*, Babine *-tṣôs*, Montagnais and Loucheux *-tṣun*, Hare *-tṣu*), it has no affinity with *-tṣœẍ*.

With regard to his strictures under No. 4, I could not, with the best of will, change what I wrote concerning the point he refers to therein. He may refuse as stoutly as he can to agree with me; further study and a little speaking knowledge of a few Déné dialects will ultimately convince him that I am right in this connection. But we must understand each other. When I said that *thé-* "hinted" at the bottom of the water, I did not mean that it denoted exclusively the ground under the same (though that prefix has frequently reference thereto); I had especially in mind the water that is near the bottom of the body of water.

Now the very words which Dr. Sapir quotes to dispose of my assertion redound against him to the extent of proving even more than I meant. I even unnecessarily qualified my statement when I said that this was the case "at least in the north," since that prefix has the same value among some of the southern tribes, as is unwittingly shown by my critic himself. He adduces Navajo *tqĕ-łī* (my *thé-łiñ*), "water-horse," which, forgetful, or unaware, of Indian exquisite accuracy in rendering linguistically the individual characteristics of natural elements, he imagines must mean literally "in-the-water horse." But I claim that he is mistaken in this. The native mind could not possibly form the concept of a horse without thinking of an animal with four feet. It is too radically exact for that when it is a question of concrete ideas. Even though the object denominated may be a real fish, the *name* given to it predicates the notion of legs and feet, which cannot be used as fins "in the water," but as means of locomotion "over" some hard substance, namely the *bottom* of the water.

This is so true that when, referring in Déné to the Eucharistic elements, I used the verbal desinence implying a personal, not material, complement, to show that these were none other than Our Lord Jesus-Christ himself, I could never prevail upon the Indians to follow me in this. They believed as firmly as I did in transsubstantiation; but their language was too strictly logical to use a verb connoting a personal complement when the *word* expressing that complement referred to a thing —a Sacrament, or the Eucharistic bread—not to a person.[1]

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really intermediate in sonancy in Hupa and was written *q* when velar, *g* when prepalatal, and sometimes *k* when postpalatal as in ʟũk kai, "white. These facts have been called to the attention of Father Morice in print previously. Ed.

[1] The following passage from a letter lately received from Rev. Father Leopold,

Dr. Sapir gives as another proof(?) that Déné *thé-* simply refers to the water, and not to the bottom of it, the fact that the Kato verb expressing the idea of washing is in *t'e'-*. In the first place, this is not to the point, since my statement did not embrace southern linguistics. Then here again his pretended proof turns against him. The Chilcotins have indeed *thénasqœs* and the Carriers *thœnnaskrœs* for "I wash"; but what kind of action do these terms represent to the native mind? How do the primitive aborigines wash? By laying over the clayey "bottom" of the shore of a sheet of water the soiled skin or blanket, on which they tread with bare feet and legs as do the wine-makers of Europe! I have witnessed this operation a number of times during my twenty-eight years' stay in British Columbia, and the Kato word referred to plainly hints that, though the natives who use it may have discarded this aboriginal method of washing, they none the less practised it formerly.

Did my diagnosis of the case stand in need of confirmation, I would have but to produce the fact that the Déné have an altogether different word to render the idea of washing either their hands or their face.

Yet, as these particular operations were very little practised formerly, the notions of washing and water bottom have remained so inseparably correlated in the Déné estimation that the term which they use to designate soap is radically identical with "mud" or "clay." The latter is called *kwœtḷés* in Chilcotin, *hwotḷœs* in Carrier, *wotḷœs* in Sékanais, and practically the same among the Eastern Déné of the north. The root, which is a primary one (hence its striking similarity), is *-tḷés*, *-tḷœs*. Now the same dialects have respectively for "soap" *la-tḷés*, *la-tḷœs* and *ta-tḷœs*, which, in the first two cases, mean "hands-mud," and in the last "eyes-mud."

The mode of washing skins or stuffs above referred to seems to have been originally unknown of the Nah'ane of the Far Northwest (the so-called Thałthan), who use in this connection a word, *œrathas'ôts*,[1] which simply implies that the action is done in the water, or with water, while

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the chief contributor to the *Navaho Dictionary*, who had not been told of my contention concerning the true etymological meaning of southern *tqě-l̄i*, is especially illuminating: "There are," he writes (April 27), "such words as *tqě-l̄i*, water-horse, and *tqě-holtsodi*, water-ox. Both are mythological animals, that live in, and walk in, or *on the bottom* of the water. This last is expressed by the prefix *tqě*." The italics are mine.

[1] Henceforth I shall represent the hiatus by an apostrophe ('), instead of an upper dot (˙) as before, and shall retain the dot exclusively for the exploded sounds *ṭ*, *ḳ*, *ḷ*, etc.

Chilcotin *thénasqœs* is analyzed: "I drag, rub (*-sqœs*) repeatedly (*-na-*)[1] over the bottom of the water (*thé-*)."

"Even in northern Athabaskan I do not find Father Morice's remark [on the real meaning of *thé-*] to apply without qualification, even if correct for Carrier." This is from Dr. Sapir's (p. 767). Let us then have recourse to his usual authorities, to which I shall add what I personally know of the Western Déné dialects. For "bottom of a body of water" the Montagnais say (with my spelling) *thèRe;* the Hares, *thèè;* the Loucheux, *thè;* the Chilcotin, Carrier and Babine, *thèR*; the Sékanais, *tcéRé*. This last is consistent with the foregoing, because in Sékanais *th* is convertible into *tc*. Ex.: "the Big Water" (the name of a British Columbia lake): Carrier, *Thû-thî;* Sékanais, *Tcû-tcî*.

As to *tha-*, my eagerness to clearly differentiate it from *thé-* induced me inadvertently to go a little too far. It is simply the equivalent in compounds—and in compounds only—of the word *thû*, *thô*, *tcû*, etc., "water," and may contribute to the formation of substantives, adjectives, verbs and adverbs. Here are a few examples:

Substantives: Navajo *thă-ba*, "shore" (for *tho-ba*, "water-edge"); Chilcotin *tha-zœl*, "soup" (for *thô-nezœl*, "water-warm"): Carrier *tha-tši*, "wave" (for *thû-tši*, lit. "water-head").

Adjectives: Chilcotin *tha-diñsat*,[2] "deep (water)," from *thô*, "water," and *nœzat*, "far off"; Babine *tha-lṭuk*, "shallow," from *thô*, "water," and *nṭuk*, "short"; Carrier *tha-sœkœz*, "spring water" (for *thû-sœkœz*, "water-cold").

Verbs: Chilcotin *tha-stnan*, "I drink (water)"; Carrier *tha-s'aih*, "I put in the water," etc.—too many verbs of that kind in all dialects for enumeration.

Adverbs: *tha-niz*, "at large on the water" (lit. "in the middle," *-niz*, "of the water," *th-*); *tha-tlat*, "at the further end (of the lake)," etc.

This element appears "in compounds only," I have said. By this I meant to controvert Dr. Sapir's statement to the effect that "in several Mackenzie Valley dialects Ath. _*t‘a_ even occurs as uncompounded noun stem" (p. 768). For this he relies, of course, on Petitot's dictionary,

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[1] Etymologically, *na-* (a contraction of *nat*, "twice") implies reduplication; but it is also frequently used simply to show that the action is not done for the first time. In those cases, it even occasionally alters somewhat the meaning of the word. Thus in Carrier *thé-tœstli* is the equivalent of "I beseech"; changed to *théna-dœstli*, it assumes the signification of "I say my prayers, I pray," because praying is an action which is normally done more than once.

[2] In Chilcotin *iñ* has the phonetic value of *in* in French *singe*, not of the same in English *sing*.

sub voce "flot." But here we have one of the cases when mere book knowledge falls short of its purpose. I never saw but one Mackenzie Valley Indian, and never spoke to her, any more than I ever addressed, or heard, a Navajo or a Hupa; yet I am positive, and if necessary will stake my reputation as a Carrier scholar (the only one which Sapir seems willing to concede) that the prefix *tha-* meaning "water"[1] as well as "flot",[2] is never used uncompounded anywhere. This would be against the morphological laws of the Déné dialects, such as my speaking knowledge of five of them and book study of many years' duration have revealed them to me. Were I not anxious to be as brief as possible, I might give my reasons for this.

In other words, you may see this terminological element written alone, as in Petitot's dictionary, or preceding independently without hyphen some other words, as in the work of the Franciscan Fathers; I insist that it cannot stand by itself, and needs the support of some other element to exist. It is to *thû*, etc., "water," what *tsé-* is to *ᴋwon*, "fire"; *tšé-* to *tši*, "head",[3] *za-* to *-zé*, "mouth," *-tze* to *-tzî*, "heart," and *ne-* or *na-* to *nèn*, *næn*, *yæn*, "earth," "ground."

But I have tarried too long on these two particles. Dr. Sapir almost ends his No. 6 criticism by adopting my suggestion that his desinence *-ał* should be *-yał*. "I now incline to think that *-yał* is correct," he admits. He is not quite so yielding in his No. 7, since he objects under that head that "there is plenty of evidence to show that Ath. *-ˣk'e*, *-k'en*, *-k'ĕn*, *-k'i* frequently refers to, or implies, paddling," which I had declared is expressed by the root *-ṭo*, Sapir's equivalent of the idea of swimming. The instances he gives absolutely fail to convince me. Nay, some of them must have appeared of very little weight even to him.

Thus when Goddard renders Chipewayan *-kĩ* (*i. e.*, *ᴋĩ*) by "to paddle a canoe, to travel by canoe," he is quite right; but this refers to locomotion, or the act of moving on the water through the action of a paddle, instead of, for instance, by swimming, *-pih*, "floating on a raft," *-tlat*, "poling up," *-thœz*, etc.

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[1] Sapir's "in the water" is not quite exact, for several dialects have a desinential letter (generally *-t*) which is locative in intent (*thût*, "in the water"; *tha-tḷat*, "at the upper end of the lake," etc.), in the same way as final *-z* denotes recess from (*thûz*, *tha-tḷaz*, etc.).

[2] Petitot gives to this French word the signification of "water," which it has sometimes in French poetry, as may be seen by the words he produces as synonymous of "vague."

[3] A somewhat extreme case of compounding into which this particle enters is *tha-na-tšé-łe'dæs'æʀ*, "I do not drink again flat on the ground," which is thus analyzed: "I am not (*-łe-*) doing (*-æʀ*) with my bent body (*æ* replaced by the hiatus *'dæs-*) a renewed (*-na-*) action connected with water (*tha-*) wherein my head (*-tšé-*) takes a part."

So is it with the example quoted from Father Legoff, whose remark is quite appropriate, and applies to all the Déné dialects that I know. *Naviguer en ramant* never meant "to paddle," any more than *j'avance en ramant*, which has the same signification. All these terms refer to locomotion which accidentally is effected by paddling, or the use of oars, on the water.

With regard to Sapír's criticism under No. 8, I merely stated the value of *-lal* in Carrier and asked whether his informant might not have misunderstood him. He shows me by examples that this was not the case. I am satisfied. This point consisted of a statement by me which was perfectly correct, and of a question which is now answered. I do not see here any room for a *corrigendum*.

I wish I could be as brief in connection with his No. 9, which he subdivided into four parts. As usual, Sapir believes that he "abundantly" proves his case by references to other dialects. Were I sure that I do not impose on the reader's patience, I feel I could, by the same process, "super-abundantly" prove the contrary. The main point at issue here is either the absence of *t-* in my critic's root stems, such as *-se*, "to cry," *-si*, "to cause," or his cutting up his words in such a way that this same letter is made to belong to the preceding pronominal crement instead of the radical desinence. Thus he wrote *-t-lat*, for *-tlat*, root for the action of floating (which is in the same category as his *-lal*); *-t-lo*, "to laugh," instead of *-tlo*, and now, in his last paper, *-t-nā* (*t'a-γit-nā*), which should be *-tnā* (*t'a-γi-tnā*, "thou drinkest").

Speaking of the root for sleeping in some dialects, he claims that "not *-tlal*, but *-lal* must be considered as the root stem," because, forsooth, the dental happens to be absent in the second persons singular and plural (*t'ī-lal* and *t'ō-lal*). He adds: "Were *-t-* part of the stem, there would be absolutely no reason for its disappearance in these forms" (p. 769). May I ask the learned doctor, firstly, whether this *t* is not found in the first person singular, or at least dual and plural, of his verb, and, secondly, why we find it in these persons of the following analogous verbs, and not in any of the second ones?

	Carrier	*Chilcotin*	*Navajo*
	I float	I am	I give him a horse
Sing.	*nœstlat*	*néstli*	*bānislŏs'*
	nīnlat	*nīnli*	*bāńlos*
	nœllat	*henli*	*yĕïlos*

Plur.	*nætsællat*	*nîtli*	*bădănîdlos'*[1]
	ŋæhlat	*nĕlli*	*bădănŏ'łos'*
	næʀællat	*qînli*	*yădeï'los*
Dual	*nîtlat*, we float (both of us)		*bănîdlos'*

To get a proper idea of the real root stem in such verbs, we must go back to those dialects which have preserved it for occasional independent use. You ask, for instance, a Carrier: "How did you come? By canoe?" *Tatqahoh hwosaînya? ᴋe pe a?*[2] He may have to answer you: *Awontuh; tlat pe æsqa;* "No, I did it by floating (on a raft)." *Lat* would here have absolutely no meaning.

In his Hupa material Goddard also gives this monosyllable instead of *tlat*; but it will be noticed that all the examples he produces as containing that root are in the third person, which, as we have seen, does not exhibit it in full. -*Tlat*, not -*lat*, is so evidently the true verbal stem that, under the transitive or causative form, the former appears in all the persons of all the tenses, both affirmative and negative, as we may see hereunder. The verb "I make him float" is conjugated as follows:

Affirmative

Present	*Past*	*Proximate Future*	*Eventual*
næstlat	*næsæstlat*	*næthîstlæl*	*nôstlat*
ŋîłtlat	*næsîłtlat*	*næthaltlæl*	*nôłtlat*
næ(yæ)łtlat	*næ(yæ)łtlat*	*ne(i)thîłtlæl*	*næ(yu)łtlat*[3]
nætsæltlat	*nætsæltlat*	*næzthîłtlæl*	*nætsultlat*
næltlat	*næsæltlat*	*næthiltlæl*	*nultlat*
næʀæ(yæ)łtlat	*næʀæ(yæ)łtlat*	*næʀe(i)thîłtlæl*	*næʀæ(yu)łtlat*
D. nîłtlat	*næsîłtlat*	*næthaltlæl*	*ŋôłtlat*

Negative

Present	*Past*	*Proximate Future*	*Eventual*
nælæzæstlat	*nælæstlal*	*nælthæzîstlæl*	*nælæzôstlat*
nælæzîłtlat	*nælîłtlal*	*nælthæzaltlæl*	*nælæzôłtlat*
nælæ(yæ)łtlat	*nælæ(yî)łtlal*	*næle(i)thîłtlæl*	*nælæ(yu)łtlat*
næltsæltlat	*næltsîłtlal*	*næltsæthîłtlæl*	*næltsultlat*
nælæzæltlat	*nælæltlal*	*nælthæzæltlæl*	*nælæzultlat*
nælæʀæ(yæ)łtlat	*nælæʀæ(yî)łtlal*	*nælæʀe(i)thîłtlæl*	*nælæʀæ(yu)łtlat*
D. nælæzîłtlat	*nælîłtlal*	*nælthæzaltlæl*	*nælæzôłtlat*

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[1] It must be borne in mind that the Déné ear perceives no difference between *d* and *t*.

[2] Of course, *ᴋe* does not literally mean "canoe," but "navigation."

[3] The particles within parentheses represent the personal complement without which a native will scarcely ever pronounce the word. This element may be omitted when the complement immediately precedes the verb; but even then it is not considered a useless redundance in Carrier. Ex.: "Paul will take all my cattle (from one place to another) by floating," *Pol smæstus tsiyauh næyułtlat*.

The desinential roots *-tlæl* of both proximate futures and *-tlal* of the negative past tense are nothing else than the general stem *-tlat*, inflected in conformity with well known, and invariably observed, laws of Carrier verbal morphology.

In the same way, the desinence of Chilcotin *néstli*, "I am," which is in Carrier *æstli*, and similarly loses its *t-* in all but the first persons singular, dual and plural, always takes it back when used substantively or as a compound element; in other words, whenever it has to play independently its rôle of a root. Thus "I am dressed" is said: *e-tæne-æstli;* "thou art dressed," *e-tæne-înli*, etc., and the compound word for clothing will be *pe-tæne-ætli*, literally "that-wherewith man one-is."

So is it with the verbal stem *-tlo*, representing the idea of laughing, which Sapir would fain have us believe to be really *-lo*, since he writes it *-t-lo*, disassociating therefrom the *t* which he unduly refers to the pronominal element of the verb. This radical quite often fills the rôle of a genuine independent noun, in fact it is a regular noun, in all the dialects: Carrier, *tlo;* Navajo, *dlo;* Chilcotin, *tlâr;* Sékanais, *tlôh;* Nah'ane, *tlok;* Montagnais, *tlô* and *dlôr;* Hare, *tlô*, *klô*, Loucheux, *tlôg*, *dlôg*.

Here are a few instances of their use: *tlo sæzîlre*, "laughter killed me,"[1]

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[1] An idiotism, the like of which we find again in Navajo. In this connection, I think it worth while to quote again from Fr. Leopold's letter (written on receipt of a copy of my review of Dr. Sapir's paper), were it only to show how, for those who are familiar with them, the dialects of the south have really preserved even minor points of the Déné morphology, such as we find it in the north.

"What you say on pages 348–49 regarding the hiatus preceding the root of the verbs of vision obtains exactly in Navaho, where this root is: *'ĭ*, *'ī*, *'īl* for the three tenses: *nesh'ī*, "I look at;" *nel'ĭ*, "I looked at;" *dínesh'īl*, "I will look at." Your remark concerning the word "wife" also applies to the Navaho word *'ad*, which is generally used with the possessive pronoun: *sha'ād*, *na'ād*, *ba'ād*, "my, your, his wife." The same hiatus occurs in Navaho before other verbal roots beginning with a vowel: *shana'a*, one of the forms for "give me." . . .

"Your remark on the double parts of the body holds good partly in Navaho: a one-eyed-man would be described as *binā'kis* (*'kūs* being a now obsolete word meaning 'half.' However, *shikhe* or *shila* is used in the singular or plural for "foot" or "hand." . . . Navaho has, as the Carrier, such personifying expressions as: *tqŏ shinilqi*, "water kills me," or "I am drowning." In like manner they say: *dichĭ shinilqi*, "hunger is killing me;" *dabă' shinilqi*, 'thirst is killing me,' to express a high degree of hunger or thirst."

Had my correspondent been familiar with the Déné dialects of the north, he would have saved himself the trouble of giving me translations of his Navajo material. There is scarcely more than one or two words (*dichĭ* for "hunger" and the future of *nesh'ī*, *i. e.*, *nec'iñ:* Chilcotin *nis'in*, Nah'ane *nes'ih*, Carrier *næs'en*) which would not be understood in the north. Thus "my, thy, his wife" is said *sæ'at*, *næ'at*, *pæ'at*, in that is, "I am dying of laughing"; *tlo ḳænne'dæsni*, "I am acting the buffoon," literally "laughter in-conformity-with I-say-many-things" (*ne'dæsni*); *tlo dîzta*, "I sit laughing"; *tlo dîjyin*, "I stand laughing"; *tlo dîzthi*, "I laugh while lying down," etc. *Lo* could not be thought of in this connection.[1]

With regard to the root for such an idea as weeping, *tso*, *tsær* in most dialects, which Sapir again writes without its *t*, here is one tense of the verb to which it belongs, together with that of a Carrier verb which is morphologically identical:

Chilcotin		*Carrier*
hæssâ, I cry		*æssi'*, I am bad
hæntsâ		*întsi'*
hætsâ		*ntsi'*
hetsâ		*tṣîntsi'*
hatsâ		*æhtsi'*
qetsâ		*ʀîntsi'*
	Dual:	*îtsi'*, both of us are bad

The Carrier for *hæssâ* is *æssær*,[2] and its past, instead of making *retsèr* as in Chilcotin, becomes *îssé*, yielding the very same root as that of Sapir's Chasta Costa; but, of course, it does not fail to assume a *t* at all its other persons. Now *tso*, *tsær* (not *so*, which means "sleet," not "weeping") is employed in exactly the same way as *tlo*. So is it with *tsi'*, not *si'*, which has no signification. It is given, under its independent form, the sense of badness, malice, wretchedness, while it is also used depreciatively in compounds, as in *ya-tsi'-dæs'aih* (see p. 570 of my last paper).

Chilcotin; Navajo *shana'a* is none other than Chilcotin *sani'añ* (last syllable as French *en*); *binā'kis* is simply Lower Carrier *bæna'ḳæz*, or *pænaḳæz*, etc.

I repeat that this information was spontaneous on the part of Fr. Leopold, who had not been told of Dr. Sapir's criticism.

As to the former's remark concerning *shikhe* (or *ci-ʀe:* Chilcotin *sæʀe*) and *shila* (Chilcotin *sæ-lla*), it applies likewise to Northern Déné. Thus a Carrier will say: *sla îlḳét*, "my hand is swollen," without using the singularizing suffix—a point I had noted long ago in the outlines of a grammar which, duly developed, may later on be published. But whenever it is necessary to specify that one hand only is intended, this particle (*-ḳæz* in Carrier, *-ḳis* in Navajo, *-qæs*, in Chilcotin, etc.) must be agglutinated to the noun. You must then say: *sla-ḳæz*, "the half of my hands." Were you to use in this case the numeral "one," as is done in our European languages, nobody would understand you.

[1] Dr. Goddard duly transcribes that *t* when he gives us the Hupa correlatives *tcwũ* and *tcwe* for *tso*.

[2] This desinential *-r* is scarcely audible at all, and many Indians, especially among the younger generation, say *tso* instead of *tsær*.

The part of Dr. Sapir's criticism where he is the most positive is in connection with syllabification. Here he declares that he "must emphatically disagree with" me (p. 771), and to show his utter disregard of what I wrote on this subject, he gives us *t‘-ā-γit-nā*, for *t‘ā-γi-tnā*; *γit-lŏ* instead of *γi-tlō*, etc., thereby absolutely disfiguring the words and granting to their pronominal inflective part that which in reality pertains to their stem. I must therefore be allowed to make a confession:

Out of sheer presumption, and imagining that my 34 years' study of the Déné language had fitted me for the task of writing thereon from mere personal knowledge, the thought of consulting a book or published paper on the subject did not as much as occur to me when I wrote my review of Sapir's able essay on Chasta Costa. Now that he "emphatically" rejects my criticism on his peculiar way of dividing syllables, which, I claim, entails a wrong idea of the personal crements no less than of the nature of the verbal stems, I must have recourse to a book. On the other hand, since it is the fashion in certain quarters to have nothing to do with the morphological notions of the North, when treating of southern dialects, I open the *Navaho Dictionary* of the Franciscan Fathers, and find, page 10 of its second part, the following caution noted in as prominent a manner as possible:

> The digraphs and trigraphs used in this alphabet are not two or three distinct consonants in juxtaposition; but represent a single sound whenever they occur initially or otherwise. We hyphenize and pronounce *bi-tqo*, not *bit-qo; bi-t'ŏ''*, not *bit'-ŏ''*, etc., a distinction which is noticed by a Navaho ear.

This remark applies to pronouns and nouns, which are always coupled together as inseparably as the various elements of a verb, since the possessive pronoun (*bi-*), for instance, cannot any more stand without the support of its noun (*tqo*) than we could dream of *t‘ā-γi-* existing without the immediate adjunction of its sense-giving stem *-tnā*. The above mentioned caution of the Navajo scholars is, therefore, just as much to the point when it is a question of verbs, as in connection with a pronominalo-substantival compound.

Hence let me tell my critic that he cannot possibly be half as emphatic concerning the accuracy of his syllabic divisions as I am with regard to their inaccuracy. His plea that his informant "was particularly careful in syllabifying" merely betrays an inexperience with Déné teachers which time will not fail to reveal. To prepare my monumental Dictionary of the Carrier language, which was destroyed by fire, I must have had, during the sixteen years that I worked on it, between 4,500 and 5,000 lessons or consultations, generally of several hours' duration.

I likewise took down vocabularies of the Chilcotin, Babine, Sékanais and Nah'ane dialects. Now I never once found a native instructor who could syllabify correctly, or in the same way as the preceding one! This task must be done by the scholar who has mastered the language sufficiently to be in a position to judge for himself, after a careful deductive process.

Perhaps the conjugation of one tense, the eventual, of the very verb of which Sapir gives one C. C. person (*t‘ā-γi-tnā*) will help opening his eyes to the truth of my contention.

Chilcotin	*Carrier*
tha-ros-tnan, I shall drink	*tha-us-naï'*
tha-roñ-tnan	*tha-ûñ-tnaï'*
tha-ro-tnan	*tha-u-tnaï'*
tha-rô-tnan	*tha-tṣu-tnaï'*
tha-roh-tnan	*tha-uh-tnaï'*
tha-qu-tnan	*tha-ʀu-tnaï'*
	Dual: *tha-ô-tnaï'*

Here we have at a glance the full root *-tnan*, *-tnaï'*. the equivalent of C. C. *-tnā*, Sékanais and Hare *-toñ*, Montagnais *-dañ*, fully distinct from the pronominal crements *-ros-*, *-us-; -roñ-*, *-ûñ-* etc., which latter are, in turn, clearly differentiated from the qualifying prefix *tha-*, which stands for *thû*, *thô*, "water," and indicates that the verb is intransitive, because it already contains in its make-up some sort of a complement. Who, with ever so slight a tincture of Déné morphology, would dream of having, for instance, *tha-st-nan*, "I drink"; *tha-înt-nan*, "thou drinkest," etc., any more than Montagnais *esd-añ*, Hare *et-on̄*?

As to Dr. Sapir's No. 10, while he admits that his verbal stem *-t‘ᴀᴄ* (or *-thæc*) is genetically related to my *-thæs*, which in all the dialects that I know of refers to the plural, he assures me that it is in Chasta Costa really used in connection with singular subjects. This is very surprising and well deserves deep investigation. Pending this, I accept his word for it, merely suggesting that this root is possibly not the equivalent of northern *-thæs*, which is strictly plural in intent, but of singular *-thih*, the desinential *-h* corresponding here to C. C. *-c*. Until I get incontrovertible evidence of it I cannot conceive of a plural form coming to express a singular concept.

With regard to Dr. Sapir's criticism under No. 11, he writes (p. 722): "Father Morice is, in my opinion, quite right." What, then, is *corrigendum* in this, his own statement or mine?

N. B.—I take this opportunity to remark that, in addition to those pointed out by Dr. Sapir, the following misprints occurred in my appreciation of his paper on the Chasta Costa language:

Page	instead of	read	Page	instead of	read
560	*t.oḳœt*	*tḷoḳœt*	567	*hwenti*	*hwentil*
561	*theʀ*	*thèʀ*	567	*łtsénœ-sœ'a*	*łtṣénœ-sœ'a*
564	*œrœl'en*	*œʀœl'en*	570	*yadœzq̣uh*	*yadœzquh*
564	*nuḳwéidîntsi'*	*nuhḳwéidîntsi'*	570	*yadœzqéh*	*yadœzq̣éh*
564	verb -*késsi'*	verb -*ḳéssi'*	570	*yatsé-sœst.s*	*yatšé-sœstłis*
567	*tsit*	*tṣit*	571	(note 3) *tsé*	*tšé*
567	(note 2) *ṭedœᴋul*	*ṭedœᴋᴜ*	571	(do.) *tsi*	*tši*[1]

A. G. MORICE, O.M.I.

SAINT BONIFACE,
MANITOBA, CANADA.

REVIEW OF "A PRE-LENAPE SITE IN NEW JERSEY:" A REPLY

IN his review of a recent publication by Dr. E. W. Hawkes and the writer ("A Pre-Lenape Site in New Jersey," *Anthropological Publications of the Museum of the University of Pennsylvania*, vol. VI, no. 3) Mr. Spier has severely criticized us for identifying the culture revealed by our excavations near Masonville, N. J. with that disclosed by the work of various investigators at Trenton. No such definite identification was intended, as may be seen from the following passages, occurring on pages 50 and 52 of the work in question.

> The presence of a large number of bannerstones in the caches at the lowest level in connection with ceremonial points of argillite, precludes any such antiquity as that claimed for the "Dweller of the Yellow Soil" by Volk, although they fall in the same intermediate period between glacial man and the modern Lenape, ***but evidently at a later date.***
>
> In conclusion, the material from our lowest and intermediate levels agrees with that of Volk's "Dweller of the Yellow Soil" in geologic position, but appears to belong to a later time within that period.

Our position, based on the material available at the time the article was written, was that the Masonville culture, while showing certain striking similarities to that found at Trenton, was more advanced and probably flourished at a later time.

A brief description of the conditions at Masonville, as contrasted with those supposedly found at Trenton, will show our reasons for taking this stand. At the former site the geologic strata were as follows: a layer of black surface soil, in which recent Indian material was found, a thick stratum of yellow soil, and a stratum of white sand. No remains were found in the yellow soil except at a single level about midway between the black soil and the white sand, where about a dozen points and some sherds of peculiar pottery occurred. At the surface of the white sand there was a well marked level of occupation, indicated by the presence of a large fire pit which had discolored the underlying stratum to a considerable depth while no similar discoloration existed in the overlying deposit. Around this fire pit were found many caches containing implements and problematical objects. At Trenton, the material was found scattered through a layer of yellow soil underlying the black surface soil. In this layer the investigators claimed to have found traces of fires and other remains, indicating that the makers of the implements had lived on the surface of the yellow soil during its deposition. Under such circumstances the artifacts found would naturally include all those types in ordinary use, and the fact that these were limited to projectile points, large chipped blades and hammerstones seemed to indicate that the ancient culture was of the simplest sort. The artifacts found at Masonville included all these types, and in addition a number of others, one of which, the bannerstone, stamped the culture as distinctly Indian. Within the types common to both, the implements from the two sites were so closely similar in material, workmanship and patina as to be quite indistinguishable. In both cases, argillite, or other closely similar minerals, were used for chipped implements to the practical exclusion of all other substances. In view of these close similarities, it appeared certain that the two cultures were in some way related. If, however, we accepted the finds at Trenton as representing the total content of the one there present, it appeared equally certain that they were not the same. We therefore assumed that the more complex Masonville culture was an outgrowth of the simpler one, and since it seems scarcely credible that the two could have existed contemporaneously within a day's walk of each other, in a region without natural barriers, we assigned this more advanced culture to a later time. The only possible alternative, that the Trenton culture marked a more recent, degenerate, form of that found at Masonville, we discarded as highly improbable.

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[1] Just to show Dr. Sapir that, especially in material such as his and mine, an author is not always responsible for the printed mistakes, I will point out the fact that his $k^{y}l$ does not correspond to my *q*, as he is made to say in note 16 of his first essay. This, at least, is a real *corrigendum*—but, I am sure, imputable to the printer alone.

The fact that the most recent map of the State Geological Survey showed the yellow soil at Masonville and Trenton as identical formations seemed to militate against this theory, for if such were the case, material from sites occupied prior to its deposition, as that of Masonville certainly was, would necessarily be older than that from sites occupied while the deposition was going on. In view of the fact that neither of us were geologists, we thought it better to leave the question of geologic identity an open one, although, as can be seen from the foregoing, any proofs of actual difference would have greatly strengthened our position. Our consulting geologist declared that the yellow soil at Masonville might be either a water deposit, or one of aeolian origin. Because of its uniform fineness, he leaned toward the latter view, and we also accepted this as allowing of a much more recent date for the culture discovered. At the same time we did not ignore the possibility that the yellow soil was a water deposit, as may be seen by referring to pages 59 and 60 of our publication.

Since the appearance of our paper, important light has been thrown upon the whole question by the publication of the results of the American Museum's work at Trenton, and by the results of certain investigations carried on during the past summer by Dr. E. W. Hawkes alone. The Museum's work at Trenton showed that the artifacts found there owed their position to natural agencies, probably water. Excavations made by Dr. Hawkes near Medford revealed an ancient camp site or workshop on the surface of a stratum of white sand overlaid by a heavy deposit of yellow sand and a surface layer of loam. The white and yellow sands from this site are quite indistinguishable from those found at the Masonville site, some eight miles away, and the strata are almost certainly the same geologically. A striking point of difference, however, and one of the greatest importance in connection with our problem, was that artifacts were found at Medford not only around fire pits on the white sand, but scattered from top to bottom of the yellow deposit. Those around the fire pits included all the types found at Masonville, even bannerstones. Those from the yellow soil on the other hand were limited to a few types of projectile points, large chipped blades and a single chipped axe. These yellow soil types were also the ones most common at the surface of the white sand, and within them it was quite impossible to distinguish between objects from the two levels. No traces of charcoal, or other signs of occupation were found in the yellow soil, and the artifacts must therefore have owed their position to natural agencies, or have been lost on the successive surfaces of the yellow deposit during the time it was being built up. The large number of objects found within a small area would seem to make the lost implement theory improbable, and if we accept that of natural agency, we must suppose the artifacts to have been originally derived from camp or village sites older than the yellow soil. These sites must therefore have been contemporaneous with or older than that here found on the surface of the white sand. If we had two camps of this culture within a short distance of each other, but at different levels, a condition might arise, in time, similar to that found here. In the course of natural erosion, pebbles, earth, and incidentally artifacts, would be carried down from the higher level and deposited upon the lower one, covering its surface and incidentally the lower of the two camp sites. Upon the artifacts thus carried the transportation would exert a selective influence, the lighter forms, and possibly those of certain shapes, being borne farther. The final area of distribution would be in the shape of a fan, pointing up hill, with the heavier material near the top and center and the more easily portable forms toward the bottom and edges. If the lower camp site was located toward the bottom of the fan, excavations there would reveal the following condition. The fan deposit would contain only those implement forms commonest on the original site and most easily portable, while the material from the camp site would include all those types found in the superposed layer and in addition such others as had been eliminated during transportation. This is exactly the condition we find at Masonville, whatever may have been the cause.

The American Museum's work at Trenton seems to have proved conclusively that the artifacts found there owe their position to natural causes. They must therefore have been derived from camp or village sites and have undergone a certain amount of selection during their transportation. The greater the force of the carrying water, and the closer the site of original deposition, the greater the chance for the presence of all the elements of the original culture. When we attempt to establish a connection between these finds and those at Masonville we are dealing, therefore, with a culture of known content and a group of scattered objects of types included within it, while from earlier information we had supposed ourselves to be dealing with two cultures of known content. If we accept the Geological Survey as authority for the identity of the yellow soil at the sites involved, we have the Trenton material derived from sites contemporaneous with or older than those found on the white sand at Masonville and Medford. It seems quite possible to account for the difference in cultural content at the two places, and at Trenton by natural selection of the sort which I have outlined, a fact which materially weakens our earlier theory that the Masonville culture was a later outgrowth of that found at Trenton. Much additional work must be done by both the archaeologist and geologist before any final decision can be made, but the weight of evidence at the present time seems to be on the side of cultural identity for the material from Trenton Masonville and Medford.

RALPH LINTON

NEW YORK CITY

INDIAN TRAP PITS ON THE MISSOURI

DR. HRDLIČKA's paper in the *Anthropologist* for December, p. 546, "Indian Trap Pits Along the Missouri," conveys to me the impression that he regards the eagle-catching pits there described as something new, but this is perhaps hardly what he means.

The practice of catching eagles by means of these pits, or by means of small enclosures built above ground and roofed over, has been well known to some ethnologists for at least twenty-five years, and, in fact, was practised by many Plains tribes up to the middle of the last century. It was the ordinary way of securing eagle feathers by the Blackfeet, Mandan, Arikara, Sioux, Cheyenne, and Arapaho, and presumably was employed by all the tribes of the plains.

It was alluded to as long ago as 1891, in the *Folk-Lore Journal*, described in 1892 in *Blackfoot Lodge Tales*, and later in other books, and, finally, is mentioned in volume II of the *Handbook of American Indians*, p. 801—Traps. All this is presumably well known to Dr. Hrdlička, and I may be to blame for not quite getting at his meaning.

GEORGE BIRD GRINNELL

NEW YORK

ANTHROPOLOGICAL NOTES

Mr. Holmes' Seventieth Anniversary

On Friday evening, December 1, a dinner was given at the Lafayette Hotel, Washington, D. C., to celebrate the seventieth anniversary of Mr. William H. Holmes, head curator of anthropology at the U. S. National Museum. On this occasion he was made the recipient of a volume of anthropological essays written for the occasion by forty-four American anthropologists. The work, which is a royal octavo, comprising 507 pages, 137 photogravure plates, and numerous text figures, is a model of the printers' and engravers' arts, and its publication in an edition of 200 copies was made possible by friends and colaborers of Mr. Holmes in the field of anthropology. The editorial work was performed by Mr. F. W. Hodge, ethnologist-in-charge of the Bureau of American Ethnology, and the papers include the following: Pomo Buildings, by S. A. Barrett; Representative Art of Primitive People, by Franz Boas; Certain Similarities in Amulets from the Northern Antilles, by Theodoor de Booy; Aboriginal Forms of Burial in Eastern United States, by David I. Bushnell, Jr.; Parallels in the Cosmogonies of the Old World and the New, by I. M. Casanowicz; Samoan Kava Custom, by William Churchill; Music in Its Relation to the Religious Thought of the Teton Sioux, by Frances Densmore; The Swan-Maiden Theme in the Oceanic Area, by Roland B. Dixon; Some South American Petroglyphs, by William Curtis Farabee; The Cliff-ruins in Fewkes Cañon, Mesa Verde National Park, Colorado, by Jesse Walter Fewkes; A Birthday Wish from Native America, by Alice C. Fletcher; The Influence of Geology on Human Development, by Gerard Fowke; The Masked Dancers of the Apache, by P. E. Goddard; A Contribution to the Archeology of Middle America, by George Byron Gordon; Ambiguity in the Taos Personal Pronoun, by John Peabody Harrington; Latest Work of the School of American Archaeology at Quirigua, by Edgar L. Hewett; The Requickening Address of the League of the Iroquois, by J. N. B. Hewitt; Certain Mounds in Haywood County, North Carolina, by George G. Heye; The Origin and Destruction of a National Indian Portrait Gallery, by F. W. Hodge; Experimental Work in American Archeology and Ethnology, by Walter Hough; Anthropology of the Chippewa, by Aleš Hrdlička; Ethnic Amalgamation, by Albert Ernest Jenks; The Use of Adobe in Prehistoric Dwellings of the Southwest, by Neil M. Judd; The Pottery of the Casas Grandes District, Chihuahua, by A. V. Kidder; Thoughts on Zuñi Religion, by A. L. Kroeber; Right and Left in Osage Ceremonies, by Francis La Flesche; Cardan's Suspension in China, by Berthold Laufer; Historical and Sociological Interpretations of Kinship Terminologies, by Robert H. Lowie; The Cult of the Ax, by George Grant MacCurdy; The Distribution of an Arawak Pendant, by Charles W. Mead; Notes on the Piegan System of Consanguinity, by Truman Michelson; Exploration of the Tremper Mound in Scioto County, Ohio, by William C. Mills; The Problem of the Red-paint People, by Warren K. Moorehead; The Supplementary Series in the Maya Inscriptions, by Sylvanus Griswold Morley; Flint Working by Ishi, by Nels C. Nelson; The Dana Estes Collection of Bronzes in the Peabody Museum of Harvard University, by Charles Peabody; Yacates of the Tierra Caliente, Michoacan, Mexico, by George H. Pepper; The Glazed Ware of Central America, with Special Reference to a Whistling Jar from Honduras, by Marshall H. Saville; An Ancient Archeological Site on the Lower St. Lawrence, by Frank G, Speck; Portraiture in Central American Art, by Herbert J. Spinden; Terms of Relationship in Timucua, by John R. Swanton; The Domain of the Aztecs and their Relation to the Prehistoric Cultures of Mexico, by Alfred M. Tozzer; The Art of the Great Earthwork Builders of Ohio, by Charles C. Willoughby; Correlations between Archeological and Culture Areas in the American Continents', by Clark Wissler. To these is added a Bibliography of Mr. Holmes' writings compiled by Ella Leary, librarian of the Bureau of American Ethnology.

Forty-two persons, practically all of whom participated in the preparation of the Anniversary volume, either as subscribers or as contributors to its contents, were present on this occasion, and the following came to Washington especially for it: Mr. Allison V. Armour, Dr. Franz Boas, Mr. George H. Pepper, Mr. C. W. Mead, Dr. H. J. Spinden, and Mr. and Mrs. Percy Jackson of New York; Mr. William Churchill and Dr. W. C. Farabee of Philadelphia; Mr. J. T. Ellis and Mr. W. E. Thomsen of Baltimore; Mr. S. G. Morley, Dr. Charles Peabody, and Mr. E. H. Thompson of Cambridge: Professor W. K. Moorehead of Andover; Mr. W. H. Jackson of Detroit, and Dr. Edgar L. Hewett of Santa Fé. Mr. Hodge acted as toastmaster and made the speech of presentation, and informal addresses of felicitation were delivered by Dr. Walcott, Miss Alice C. Fletcher, Professor Moorehead, Professor Boas, Mr. Armour, Dr. Peabody, Dr. Fewkes, Professor Mitchell Carroll, Mr. W. H. Jackson, and Mr. E. H. Thompson.

THE MEANING OF "TUSAYAN".

In a recent letter to Mr. F. W. Hodge, Father Berard, O.F.M., of St. Isabel's, Lukachukai, Arizona, says:

> The resemblance between the Navaho *Tasaun* or *Zilh Tasaun*, "the country of isolated buttes," and *Tusayan* would seem to be fortuitous. . . . I take it that *Tasaun* or *Zilh Tasaun* stands for *dzil da' sa'â*, which signifies monocline or a lone butte or mountain. *da* is not the sign of the plural, but an adverbial abbreviation for *a'da'*, "the drop off" of a mountain ridge, in this instance the lone mountain at the mouth of Grand Cañon in the Supai District. This would hardly cover *Tusayan* or its people for which the Navaho use *a'yakini*, a word meaning "the people living in underground houses, or houses you enter from above," which makes no reference to their topography.
>
> The Navaho names of the Hopi Pueblos are:
>
> *ōzai'*, Oraibi, the meaning being unknown to the present generation.
> *ozai' biyazhe*, "Little Oraibi," Moencopie.
> *nadak'â k'aedilyedi*, "where they grow cotton," Moencopie, Wash.
> *nā sha'shi* "bear enemies," the Hano.
> *atqa'kini*, "middle houses," Sichomovi.
> *kíts'ili*, "broken pottery," Shumopovi.
> *kina'zt'ii*, "a line of houses," Shipauolovi.
> *tse'tsok'ĭd*, "boulder hill," Mishongnovi.
> *a'yakini* or *ni'yakini*, "underground houses," Walpi.
> *tqa'lahoghan*, "mythical water houses," Awatobi, which is extinct and has been accepted as mythical.
>
> The *z* in *ozai* was probably substituted for *r* which is wanting in Navaho. In my opinion *a'yakini* was never meant to refer to cave dwellers, unless possibly in jest. The kiva or estufa is hardly meant either as such rooms were familiar sights among other *kis'ani* or Pueblos. I would suggest rather that in the early days the Hopi built largely underground houses on mesas which lent themselves better for retreat. A Navaho distrusts an underground dwelling, saying "Why bury yourself alive?"
>
> The migration of the Navaho from *dinetga*, Navaho Land (in the Tierra Amarilla country) into their present habitat does not antedate Spanish occupation. Indeed, I should draw this period well up towards the Pueblo Rebellion in 1680 or later, of which reminiscences remain in a tradition of the descendants of the murderers of the priests at Jemez. Previous to this, tradition is so obscured that it was possible for them to introduce the creation of the horse, sheep, and cow into legend and prayer—wishing thus to antedate the Spaniard—and overrule the protests of more conservative singers who protested their Spanish origin. Yet it is evident that after the migration much Pueblo material was introduced into tribal history through refugees and captives."

THE JOHN G. WHITE COLLECTION of Folklore, Oriental and Mediaeval Literature, and Archaeology, now owned by the Cleveland Public Library, comprises 30,000 volumes and pamphlets, with additions at the rate of 2,000 or 3,000 annually; and is available for loan to those interested, whether residents of Cleveland or not. The material is now in order, and a librarian in charge.

In the general field of folklore, the material is large. It includes the chief magazines such as *Mélusine*, *Revue des Traditions Populaires*, *La Tradition*, *Ons Volksleven*, *Folk-lore*, *Dania*, *Archives Suisses des Traditions Populaires*, *Archivio per le Tradizioni Popolari*, *Volkskunde*, *etc.*

Local folklore has several thousand volumes and pamphlets, German being particularly strong. Other subjects of note are ballads (a very large collection), fables, proverbs, gypsy, and mediaeval romances and legends. There is an excellent collection on Faust, including many German and Dutch chapbooks. Tyll Eulenspiegel and the Seven Wise Masters, Rübezahl and the Norse sagas should also be mentioned. On witchcraft there is less material, but there are some rare and early works, and pamphlets infrequently met with, especially in German. Alchemy and astrology have not been purchased to any large extent. The collection on the American Indians has developed recently. There are also, of course, books on plant and animal lore and other similar matters, and a number of saints' legends, chapbooks, and broadside ballads.

Much in the other portions of this collection will also be of interest. Oriental literature is perhaps the most notable feature. In all, 140 languages from all parts of the world are represented, Sanskrit, Arabic and Persian being very numerous. Oriental history, especially of India; mediaeval literature, present in great abundance (except that purely linguistic material, and the philological journals have not, as a rule, fallen within the scope of the collection); archaeology, chiefly Asiatic, including a long series of the publications of the Archaeological Survey of India and neighboring countries; Assyriology, Egyptology (all the principal archaeological serials being found), Mexican and Maya picture writing, China and Japan, early Irish and Welsh literature, are some other features. Ethnology and early travel are important features, a number of the geographical publishing societies having been added of late. Finally, in catalogues of manuscripts in European and Oriental libraries, the White collection stands among the first three or four in the country.

While no printed lists are in existence, any desired book can be found readily, and loans will gladly be made. Applications should be made if possible through the library of the institution with which the applicant is connected; those not in a position to comply with this requirement should state their case. The period for which books are loaned is ordinarily two weeks, with the privilege of renewal for two weeks more; but other arrangements may be made if need warrants. All communications should be addressed to the librarian, Public Library, Cleveland, Ohio.

On February 28 the Geologic Society of London again had the opportunity of discussing the Piltdown man. The late Charles Dawson had discovered further remains about a mile away from the original locality, but in gravel of the same age. These, which consist of a molar from the lower jaw, a fragment of the occiput, and a part of a frontal including some of the orbital margin, were laid before the meeting by Dr. A. Smith Woodward.

Mr. D. Jenness has prepared some Eskimo folklore texts, chiefly from northern Alaska, during the past few months, accompanied by translations, besides editing a number of folklore stories collected in English between Alaska and Coronation Gulf. A large series of cat's cradle figures, treated comparatively, has been worked up for publication. At the present time he is working on a general account of the Copper Eskimo and their country, from notes gathered during his residence amongst them as a member of the Canadian Arctic Expedition. Mr. Jenness' Eskimo studies are to be published by the Geological Survey of Canada.

Miss Frances Densmore returned to Washington the first week in January from further studies of Indian music among the Indians of Dakota for the Bureau of American Ethnology. Mr. Francis La Flesche returned in February from field work in Oklahoma.

Dr. Leo J. Frachtenberg, of the Bureau of American Ethnology, returned to Washington on February 4 after an absence of nearly two and a half years devoted to investigations, mainly linguistic, of the Indian tribes of Washington and Oregon.

Dr. Aleš Hrdlička of the U. S. National Museum delivered two lectures in January before the faculty and advanced students of the University of Virginia on "Anthropology" and "The Evolution of Man."

Mr. William Hubbs Mechling has been appointed assistant curator of North American Ethnology in the department of anthropology of the Field Museum of Natural History, Chicago. Mr. Mechling is at present in Ottawa, Canada, being engaged on the working up of his material on the Malecite and Micmac Indians. He will assume his duties in Chicago on July 1.

Dr. F. C. Cole, assistant curator in the Field Museum of Natural History, Chicago, will deliver ten lectures in the east during the latter part of March at the University Museum, Philadelphia; the Anthropological Society of Washington; Columbia University, New York; and the Brooklyn Institute of Arts and Sciences.

Mr. E. W. Gifford of the anthropological department of the University of California has returned from a trip of investigation of the kinship system and social organization of the Indians of southern California.

Dr. E. W. Hawkes, of Philadelphia, has been appointed to the anthropological staff of the Milwaukee Public Museum.

By Executive Order of January 3, 1917, amended by executive order of February 6, 1917, President Wilson has withdrawn a tract of land on the northern shore of Norton Bay, Alaska, and certain adjacent islands, "for the use of the United States Bureau of Education and of the natives of indigenous Alaskan race."

By Executive Order of January 11, 1917, Prexident Wilson has ordered that certain lands adjacent to the ruins of the historic Piro pueblo of Gran Quivira, in New Mexico, be withdrawn "for classification and pending determination as to the advisability of including such lands within the Gran Quivira National Monument."

Professor Edward Burnett Tylor, the English anthropologist, author of *Primitive Culture* and other well-known books, died at Wellington, Somerset, on January 2. The next number of the *Anthropologist* will contain an extensive account of Professor Tylor's activities.

Mr. John P. Harrington announces that he has found genetic relationship between Washoe and Chumashan.

The next Annual Meeting of the American Anthropological Association will be held in the new auditorium of the University Museum, 33d and Spruce streets, Philadelphia, December 27–29, 1917.

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By A. L. KROEBER

A WAY of thought characteristic of our western civilization has been the formulation of complementary antitheses, a balancing of exclusive opposites. One of these pairs of ideas with which our world has been laboring for some two thousand years is expressed in the words *body* and *soul*. Another couplet that has served its useful purpose, but which science is now often endeavoring to rid itself of, at least in certain aspects, is the distinction of the *physical* from the *mental*. A third discrimination is that of the *vital* from the *social*, or in other phraseology, of the *organic* and the *cultural*. The implicit recognition of the difference between organic qualities and processes and social qualities and processes is of long standing. The formal distinction is however recent. In fact the full import of the significance of the antithesis may be said to be only dawning upon the world. For every occasion on which some human mind sharply separates organic and social forces, there are dozens of other times when the distinction between them is not thought of, or an actual confusion of the two ideas takes place.

One reason for this current confusion of the organic and the social is the predominance, in the present phase of the history of thought, of the idea of evolution. This idea, one of the earliest, simplest, and also vaguest ever attained by the human mind, has received its strongest ground and fortification in the domain of the organic; in other words, through biological science. At the same time, there is an evolution, or growth, or gradual development, apparent also in other realms than that of plant and animal life. We have theories of stellar or cosmic evolution; and there is obvious, even to the least learned, a growth or evolution of civilization. In the nature of things there is little danger of the carrying over of the Darwinian or post-Darwinian principles of the evolution of life into the realm of burning suns and lifeless nebulae. Human civilization or progress, on the other hand, which exists only in and through living members of the species, is so unmistakably similar to the evolution of plants and animals, that it has been inevitable that there should have been sweeping applications of the principles of organic development to the facts of cultural growth. This of course is reasoning by analogy, or arguing that because two things resemble each other in one point they will also be similar in others. In the absence of knowledge, such assumptions are justifiable as assumptions. Too often, however, their effect is to predetermine mental attitude, with the result that when the evidence begins to accumulate which could prove or disprove the assumption based on analogy, this evidence is no longer viewed impartially and judiciously, but is merely distributed and disposed of in such a way as not to interfere with the established conviction into which the original tentative guess has long since turned.

This is what has happened in the field of organic and social evolution. The distinction between them, which is so obvious that to former ages it seemed too commonplace to remark upon, except incidentally and indirectly, has been largely obscured in the last fifty years through the hold which thoughts connected with the idea of organic evolution have had on minds of the time. It even seems fair to say that this confusion has been greater and more general among those to whom study and scholarship are a daily pursuit than to the remainder of the world.

And yet many aspects of the difference between the organic and that in human life which is not organic, are so plain that a child can grasp them, and that all human beings, including the veriest savages, constantly employ the distinction. Everyone is aware that we are born with certain powers and that we acquire others. There is no need of argument to prove that we derive some things in our lives and make-up from nature through heredity, and that other things come to us through agencies with which heredity has nothing to do. No one has yet been found to assert that any human being is born with an inherent knowledge of the multiplication table; nor, on the other hand, to doubt that the children of a negro are born negroes through the operation of hereditary forces. Some qualities in every individual are however clearly debatable ground; and when the development of civilization as a whole and the evolution of life as a whole are compared, the distinction of the processes involved has too often been allowed to lapse.

Some millions of years ago, it is currently taught, natural selection, or some other evolutionary agency, first caused birds to appear in the world. They sprang from reptiles. Conditions were such that the struggle for existence on the earth was hard; while in the air there were safety and room. Gradually, either by a series of almost imperceptible gradations through a long line of successive generations, or by more marked and sudden leaps in a shorter period, the group of birds was evolved from its reptilian ancestors. In this development, feathers were acquired and scales lost; the grasping faculty of the front legs was converted into an ability to sustain the body in the air. The advantages of resistance enjoyed by a cold-blooded organization were given up for the equivalent or greater compensation of the superior activity that goes with warm-bloodedness. The net result of this chapter of evolutionary history was that a new power, that of aerial locomotion, was added to the sum total of faculties possessed by the highest group of animals, the vertebrates. The vertebrate animals as a whole, however, were not affected. The majority of them are as totally without the power of flight as their ancestors were millions of years ago. The birds, in turn, had lost certain faculties which they once possessed, and presumably would still possess were it not for the acquisition of their wings.

In the last few years human beings have also attained the power of aerial locomotion. But the process by which this power was attained, and its effects on the species, are as utterly different from those which characterized the acquisition of flight by the first birds as it is possible for them to be. Our means of flying are outside of our bodies. A bird is born with a pair of wings, but we have invented the aeroplane. The bird renounced a potential pair of hands to get his wings; we, because our new faculty is not part of our congenital make-up, keep all the organs and capacities of our forefathers but add to them the new ability. The process of the development of civilization is clearly one of accumulation: the old is retained, in spite of the incoming of the new. In organic evolution, the introduction of new features is generally possible only through the loss or modification of existing organs or faculties.

In short, the growth of new species of animals takes place through, and in fact consists of, changes in their organic constitution. As regards the growth of civilization, on the other hand, the one example cited is sufficient to show that change and progress can take place through an invention without any such constitutional alteration of the human species.

There is another way of looking at this difference. It is clear that as a new species originates, it is derived wholly from the individual or individuals that first showed the particular traits distinguishing the new species. When we say that it is derived from these individuals we mean, literally, that it is descended. In other words, the species is composed only of such individuals as contain the blood of particular ancestors. Heredity is thus the indispensable means of transmission. When, however, an invention is made, the entire human race is capable of profiting thereby. People who have not the slightest blood kinship to the first designers of aeroplanes can fly and are flying today. Many a father has used, enjoyed, and profited by the invention of his son. In the evolution of animals, the descendant can build upon the inheritance transmitted to him from his ancestors, and may rise to higher powers and more perfect development; but the ancestor is, in the very nature of things, precluded from thus profiting from his descendant. In short, organic evolution is essentially and inevitably connected with hereditary processes; the social evolution which characterizes the progress of civilization, on the other hand, is not, or not necessarily, tied up with hereditary agencies.

The whale is not only a warm-blooded mammal, but is recognized as the remote descendant of carnivorous land animals. In some few million years, as such genealogies are usually reckoned, this animal lost his legs for running, his claws for holding and tearing, his original hair and external ears that would be useless or worse in water, and acquired fins and fluke, a cylindrical body, a layer of fat, and the power of holding his breath. There was much that the species gave up; more, on the whole, perhaps than it gained. Certainly some of its parts have degenerated. But there was one new power that it did achieve: that of roaming the ocean indefinitely.

The parallel and also contrast is in the human acquisition of the identical faculty. We do not, in gradual alteration from father to son, change our arms into flippers and grow a tail. We do not enter the water at all to navigate it. We build a boat. And what this means is that we preserve our bodies and our natal faculties intact, unaltered from those of our fathers and remotest ancestors. Our means of marine travel is outside of our natural endowment. We make it and use it: the original whale had to turn himself into a boat. It took him countless generations to attain to his present condition. All individuals that failed to conform to type left no offspring; or none that went into the blood of the whales of today.

Again, we may compare human and animal beings when groups of them reach a new and arctic environment, or when the climate of the tract where the race is established slowly becomes colder and violently colder. The non-human mammal species comes to have heavy hair. The polar bear is shaggy, his Sumatran relative sleek. The arctic hare is enveloped in soft fur; the jack-rabbit in comparison is shabbily thin and moth-eaten. Good furs come from the far north, and they lose in richness, in quality, and in value, in proportion as they are stripped from animals of the same species that inhabit milder regions. And this difference is racial, not individual. The jack-rabbit would quickly perish with the end of summer in Greenland; the caged polar bear suffers from temperate warmth within the massive coat which nature has fastened on him.

Now there are people who look for the same sort of inborn peculiarities in the Eskimo, the Samoyed, and the Yahgan; and find them, because they look for them. That the Eskimo is furry, no one can assert: in fact, we are hairier than he. But it is asserted that he is fat-protected—like the blubber-covered seal that he lives on; and that he devours vast quantities of heat-giving meat and oil because he must. The true amount of his fat, compared with that of other human beings, remains to be ascertained. He probably has more than the European; but probably no more than the normal full-blood Samoan and Hawaiian from under the tropics. And as to his diet, if this is seal and seal and seal all winter long, it is not from any congenital craving of his stomach, but because he does not know how to get himself anything else. The Alaskan miner, and the arctic and antarctic explorer, do not guzzle blubber. Wheat-flour, eggs, coffee, sugar, potatoes, canned vegetables—whatever the exigencies of their vocation, and the cost of transportation permit—make up their fare. The Eskimo is only too anxious to join them; and both he and they can thrive as well on the one diet as on the other.

In fact, what the human inhabitant of intemperate latitudes does, is not to develop a peculiar digestive system, any more than he grows hair. He changes his environment, and thereby is able to retain his original body unaltered. He builds a closed house, which keeps out the wind and retains the heat of his body. He makes a fire or lights a lamp. He skins a seal or a caribou of the furry hide with which natural selection or other processes of organic evolution have endowed these beasts; he has his wife make him a shirt and trousers, boots and gloves, or two sets of them; he puts them on; and in a few years, or days, he is provided with the protection which it took the polar bear and the arctic hare, the sable and the ptarmigan, untold periods to acquire. What is more, his baby, and his baby's baby, and his hundredth descendant are born as naked, and unarmed physically, as he and his hundredth ancestor were born.

That this difference in method of resisting a difficult environment, as followed respectively by the polar bear species and the human Eskimo race, is absolute, need not be asserted. That the difference is deep, is unquestionable. That it is as important as it is often neglected, it is the object of this essay to establish.

It has long been the custom to say that the difference is that between body and mind; that animals have their physiques adapted to their circumstances, but that man's superior intelligence enables him to rise superior to such lowly needs. But this is not the significant point of the difference. It is true that without the much greater mental faculties of man, he could not achieve the attainments the lack of which keeps the brute chained to the limitations of his anatomy. But the greater human intelligence in itself does not cause the differences that exist. This psychic superiority is only the indispensable condition of what is peculiarly human: civilization. Directly, it is the civilization in which every Eskimo, every Alaskan miner or arctic discoverer is reared, and not any greater inborn faculty, that leads him to build houses, ignite fire, and wear clothing. The distinction between animal and man which counts is not that of the physical and mental, which is one of relative degree, but that of the organic and social, which is one of kind. The beast has mentality, and we have bodies; but in civilization man has something that no animal has.

That this distinction is actually something more than that of the physical and mental, appears from an example that may be chosen from the non-bodily: speech.

On the surface, human and animal speech, in spite of the enormously greater richness and complexity of the former, are much alike. Both express emotions, possibly ideas, in sounds formed by bodily organs and understood by the hearing individual. But the difference between the so-called language of brutes and that of men is infinitely great; as a homely illustration will set forth.

A newly-born pup is brought up in a litter of kittens by a fostering cat. Familiar anecdotes and newspaper paragraphs to the contrary, the youngster will bark and growl, not purr or miaow. He will not even try to do the latter. The first time his toe is stepped on, he will whine, not squeal, just as surely as when thoroughly angered he will bite as his never-beheld mother did, and not even attempt to claw as he has seen his foster-mother do. For half his life seclusion may keep him from sight or sound or scent of another dog. But then let a bark or a snarl reach him through the restraining wall, and he will be all attention—far more than at any voice ever uttered by his cat associates. Let the bark be repeated, and interest will give way to excitement, and infallibly he will answer in kind, as certainly as, put with a bitch, the sexual impulses of his species will manifest themselves. It cannot be doubted that dog speech is as ineradicably part of dog nature, as fully contained in it without training or culture, as wholly part of the dog organism, as are teeth or feet or stomach or motions or instincts. No degree of contact with cats, or deprivation of association with his own kind, can make a dog acquire cat speech, or lose his own, any more than it can cause him to switch his tail instead of wagging it, to rub his sides against his master instead of leaping against him, or to grow whiskers and carry his drooping ears erect.

Let us take a French baby, born in France of French parents, themselves descended for numerous generations from French-speaking ancestors. Let us, at once after birth, entrust the infant to a mute nurse, with instructions to let no one handle or see her charge, while she travels by the directest route to the interior heart of China. There she delivers the child to a Chinese couple, who legally adopt it, and rear it as their son. Now suppose three or ten or thirty years passed. Is it needful to discuss what the growing or grown Frenchman will speak? Absolutely not a word of French; and absolutely pure Chinese, without a trace of accent and with Chinese fluency; and nothing else.

It is true that there is a common delusion, frequent even among educated people, that some hidden influence of his French-talking ancestors will survive in the adopted Chinaman: that it is only necessary to send him to France with a batch of real Chinamen, and he will acquire his mother's tongue with appreciably greater facility, fluency, correctness, and naturalness than his Mongolian companions. That a belief is common, however, is as likely to stamp it a common superstitition as a common truth. And any reasonable biologist, in other words, an expert qualified to speak of heredity, will pronounce this favored answer to this problem in heredity, pure superstition. He might merely choose a politer phrase.

Now there is something deep going here. No amount of association with Chinese would turn our young Frenchman's eyes from blue to black, or slant them, or flatten his nose, or coarsen or stiffen his wavy, oval-sectioned hair; and yet his speech is totally that of his associates, in no measure that of his blood kin. His eyes and his nose and his hair are his from heredity; his language is non-hereditary—as much so as the length to which he allows his hair to grow, or the hole which, in conformity to fashion, he may or may not bore in his ears. It is not that speech is mental and facial proportions physical; the distinction that has meaning and use is that human language is non-hereditary and social, eye-color and nose-shape hereditary and organic. By the same criterion, dog speech, and all that is vaguely called the language of animals, is in a class with men's noses, the proportions of their bones, the color of their skin, and the slope of their eyes, and not in a class with any human idiom. It is inherited, and therefore organic. By a human standard, it is not really language at all, except by the sort of metaphor that speaks of the language of the flowers.

It is true that now and then a French child would be found that under the conditions of the experiment assumed, would learn Chinese more slowly, less idiomatically, and with less power of expression, than the average Chinaman. But there would also be French babies, and as many, that would acquire the Chinese language more quickly, more fluently, with richer power of revealing their emotions and defining their ideas, than the normal Chinese. These are individual differences, which it would be absurd to deny, but which do not affect the average, and are not to the point. One Englishman speaks better English, and more of it, than another, and he may also, through precocity, learn it much sooner; but one talks English no more and no less truly than the other.

There is one form of animal expression in which the influence of association has sometimes been alleged to be greater than that of heredity. This is the song of birds. There is a good deal of conflicting opinion, and apparently of evidence, on this point. Many birds have a strong inherent impulse to imitate sounds. It is also a fact that the singing of one individual stimulates the other—as with dogs, wolves, cats, frogs, and most noisy animals. That in certain species of birds capable of a complex song the full development will not often be reached in individuals raised out of hearing of their kind, may probably be admitted. But it seems to be clear that every species has a song or call distinctively its own; that this minimum is attainable without association by every normal member of the singing sex, as soon as conditions of age, food, and warmth are proper, and the requisite stimulus of noise, or silence, or sex development, is present. That there has been serious conflict of opinion as to the nature of bird song, will ultimately be found to be chiefly due to the pronouncement of opinions on the matter by those who read their own mental states and activities into animals—a common fallacy that every biological student is now carefully trained against at the outset of his career. In any event, whether one bird does or does not in some degree "learn" from another, there is not a fragment of even alleged evidence that bird song is a tradition, that like human speech or human music it accumulates and develops from age to age, that it is inevitably altered from generation to generation by fashion or custom, and that it is impossible for it ever to remain the same: in other words, that it is a social thing or due to a process even remotely akin to those affecting all constituents of human civilization.

It is also true that there is in human life a series of utterances that are of the type of animal cries. A man in pain moans without purpose of communication. The sound is literally pressed from him. A person in supreme fright may shriek. We know that his cry is instinctive, unintended, what the physiologist calls a reflex action. The true shriek is as liable to escape the victim pinned before the approaching engineerless train, as him who is pursued by thinking and planning enemies. The woodsman crushed by a rock forty miles from the nearest human being, will moan like the run-over city dweller surrounded by a crowd waiting for the speeding ambulance. Such cries are of a class with those of animals. In fact, to really understand the "speech" of brutes, we must think ourselves into a condition in which our utterances would be totally restricted to such instinctive cries—"inarticulate" is their general though often inaccurate designation. In any exact sense, they are not language at all.

This is precisely the point. We undoubtedly have certain activities of utterance, certain faculties and habits of sound production, that are truly parallel with those of animals; and we also have something more that is entirely different and without parallel among the animals. To deny that something purely animal underlies human speech, is fatuous; but it would be equally narrow to believe that because our speech springs from an animal foundation, and originated in this foundation, it therefore is nothing but animal mentality and utterances greatly magnified. A house may be built on rock; without this base it might be impossible for it to have been erected; but no one will maintain that therefore the house is nothing but improved and glorified stone.

As a matter of fact, the purely animal element in human speech is small. Apart from laughter and crying, it finds rare utterance. Our interjections are denied by philologists as true speech, or at best but half admitted. It is a fact that they differ from full words in not being voiced, generally, to convey a meaning—nor to conceal one. But even these particles are shaped and dictated by fashion, by custom, by the type of civilization to which we belong, in short by social and not by organic elements. When I drive the hammer on my thumb instead of on the head of the nail, an involuntary "damn" may escape me as readily if I am alone in the house, as if companions stand on each side. Perhaps more readily. So far, the exclamation does not serve the purpose of speech and is not speech. But the Spaniard will say "carramba" and not "damn";

and the Frenchman, the German, the Chinaman, will avail himself of still different expression. The American says "outch" when hurt. Other nationalities do not understand this syllable. Each people has its own sound; some even two—one used by men and the other by women. A Chinaman will understand a laugh, a moan, a crying child, as well as we understand it, and as well as a dog understands the snarl of another dog. But he must learn "outch," or it is meaningless. No dog, on the other hand, ever has given utterance to a new snarl, unintelligible to other dogs, as a result of having been brought up in different associations. Even this lowest element of human speech, then, this involuntary half-speech of exclamations, is therefore not indeed caused but at any rate shaped by social influences.

Herodotus tells of an Egyptian king, who, wishing to ascertain the parent tongue of humanity, had some infants brought up in isolation from their own kind, with only goats as companions and for sustenance. When the children, grown older, were revisited, they cried the word "bekos," or, subtracting the ending which the normalizing and sensitive Greek could not endure omitting from anything that passed his lips, more probably "bek." The king then sent to all countries to learn in what land this vocable meant something. He ascertained that in the Phrygian idiom it signified bread, and, assuming that the children were crying for food, concluded that they spoke Phrygian in voicing their "natural" human speech, and that this tongue must therefore be the original one of all mankind. The king's belief in an inherent and congenital language of man, which only the blind accidents of time had distorted into a multitude of idioms, may seem simple; but naïve as it is, inquiry would surely reveal crowds of civilized people still adhering to it.

This however is not our moral to the tale. That lies in the fact that the one and only word attributed to the children, "bek," was, if the story has any authenticity whatsoever, only a reflection or imitation—as the commentators of Herodotus long since conjectured—of the cry of the goats who were the children's only associates and instructors. In short, if it is allowable to deduce any inference from so apocryphal an anecdote, what it proves is that there is no natural and therefore no organic human language.

Thousands of years later another sovereign, the Mogul emperor Akbar, repeated the experiment with the intent of ascertaining the "natural" religion of mankind. His band of children were shut up in a house. When, the necessary time having elapsed, the doors were opened in the presence of the expectant and enlightened ruler, his disappointment was great: the children trooped out as dumb as deaf-mutes. Faith dies hard, however; and we may suspect that it would take a third trial, under modern chosen and controlled conditions, to satisfy some natural scientists that speech, for the human individual and for the human race, is wholly an acquired and not a hereditary thing, entirely outward and not at all inward—a social product and not an organic growth.

Human and animal speech, then, though one roots in the other, are in their nature of a different order. They resemble each other only as the flight of a bird and of an aeronaut are alike. That the analogy between them has frequently deceived, proves only the guilelessness of the human mind. The operative processes are wholly unlike; and this, to him who is desirous of understanding, is far more important than the similarity of effect. The savage and the peasant who cure by cleaning the knife and leaving the wound unattended, have observed certain indisputable facts. They know that cleanness aids, dirt on the whole impedes recovery. They know the knife as the cause, the wound as the effect; and they grasp, too, the correct principle that treatment of the cause is in general more likely to be effective than treatment of the symptom. They fail only in not inquiring into the process that may be involved. Knowing nothing of the nature of sepsis, of bacteria, of the excretion of organic poisons, those agencies of putrefaction and retardation of healing, they fall back on agencies more familiar to themselves, and use, as best they may, the process of magic intertwined with that of medicine. They carefully scrape the knife; they oil it; they keep it bright. The facts from which they work are correct; their logic is sound enough; they merely do not distinguish between two irreconcilable processes—that of magic and that of physiological chemistry—and apply one in place of another. The student of today who reads the civilizationally moulded mind of men into the mentality of a dog or ape, or who tries to explain civilization—that is, history—by organic factors, commits an error which is, it is true, less antiquated and more in fashion, but of the same kind and nature, and of similarly disastrous consequence.

It is only in small measure a question of high and low as between man and animal. Many purely instinctive activities of the beasts lead to far more complex and difficult achievements than some of the analogous customs of this or that human nation. The beaver is a far better architect than many a savage tribe. He fells larger trees, he drags them farther, he builds a closer house; he constructs it both below and above water; and he does what many nations never attempt to do: he makes himself a suitable topography for a habitat by erecting a dam. But the essential point is not that after all a man can do more than a beaver, or a beaver as much as a man; it is that all that a beaver accomplishes he does by one means, and a man by another. The rudest savage, who builds but a shack of a wind-pierced hut, can be taught, innumerable times has been taught, to saw and nail together boards, to mortar stone on stone, to sink foundations, to rear an iron frame. All human history concerns itself primarily with just such changes. What were the ancestors of ourselves, of us steel-building Europeans and Americans, but hut-dwelling savages a few thousand years ago—a period so short that it may barely suffice for the formation of an occasional new species of organism? And on the other side, who would be so rash as to affirm that one generation or a hundred or ten thousand of example and instruction would in the least measure convert the beaver from what he is into a carpenter or a bricklayer—or, allowing for his physical deficiency in the lack of hands, into a planning engineer?

The utter divergence between social and organic forces will perhaps never be fully grasped until the mentality of the so-called social insects, the bees and ants, is thoroughly realized. Social the ant is, in the sense that she associates; but she is so far from being social in the sense of possessing civilization, of being influenced by non-organic forces, that she would better be known as the anti-social animal. The marvelous powers of the ant cannot be underestimated. There is no one to whom the full exploitation of their understanding will be of more service than to the historian. But he will not use this understanding by applying his knowledge of ant mentality to man. He will use it to fortify and render precise by intelligent contrast, his conception of the agencies that mould human civilization. Ant society is as little a true society, to any student imbued with the rudiments of the historical spirit, as the demon worshiped in a grotesque pagan idol is to the devout Catholic or Mohammedan the true God; as a deliberate caricature is a portrait; as a spurious imitation is identical with the counterfeited ware.

Take a couple of ant eggs of the right sex—unhatched eggs, freshly laid. Blot out every individual and every other egg of the species. Give the pair a little attention as regards warmth, moisture, protection, and food. The whole of ant "society," every one of the abilities, powers, accomplishments, and activities of the species, each "thought" that it has ever had, will be reproduced, and reproduced without diminution, in one generation. But place on a desert island or in a circumvallation two or three hundred human infants of the best stock from the highest class of the most civilized nation; furnish them the necessary incubation and nourishment; leave them in total isolation from their kind; and what shall we have? The civilization from which they were torn? One tenth of it? No, not any fraction; nor a fraction of the civilizational attainments of the rudest savage tribe. Only a pair or a troop of mutes, without arts, knowledge, fire, without order or religion. Civilization would be blotted out within these confines—not disintegrated, not cut to the quick, but obliterated in one sweep. Heredity saves for the ant all that she has, from generation to generation. But heredity does not maintain, and has not maintained, because it cannot maintain, one particle of the civilization which is the one specifically human thing.

The mental activity of the animals is instinctive; the content, at least, of our own minds comes to us through tradition, in the widest sense of the word. Instinct is what is "pricked in"; an unalterable pattern inherent in the goods; indelible and inextinguishable, because the design is nothing but the warp and the woof, coming ready-made from the loom of heredity.

But tradition, what is "given through," handed along, from one to another, is only a message. It must of course be carried; but the messenger after all is extrinsic to the news. So, a letter must be written; but as its significance is in the meaning of the words, as the value of a note is not in the fiber of the paper but in the characters inscribed on its surface, so tradition is something superadded to the organisms that bear it, imposed upon them, external to them. And as the same shred can bear any one of thousands of inscriptions, of the most diverse force and value, and can even be tolerably razed and reinscribed, so it is with the human organism and the countless contents that civilization can pour into it. The essential difference between animal and man, in this illustration, is not that the latter has finer grain or the chaster quality of material; it is that his structure and nature and texture are such that he is inscribable, and that the animal is not. Chemically and physically, there is but little difference between a lump of pulp and a sheet of paper. Chemically and physically, it is of the slightest consequence to trouble about such minute difference. But chemically and physically there is still less difference between the treasury note stamped with "one" and that stamped with "one thousand"; and yet less between the check with an honored signature and that written with the same pen, the same ink, the same strokes even, by a forger. The difference that counts between the valid and the counterfeit check, is not the broader or the narrow line, the continuous curve of a letter in place of the broken one, but the purely social one that one signer has a valid account in the bank and the other has not; which fact is surely extrinsic to the paper and even to the ink upon it.

Exactly parallel to this is the relation of the instinctive and traditional, the organic and the social. The animal, so far as social influences are concerned, is as unsuitable as a dish of porridge

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is for writing material; or when like the beach sand, it is inscribable, by domestication, it can retain no permanent impression, as a species, and lends itself to no use. Hence it has no society, and therefore no history. Man, however, comprises two aspects: he is an organic substance, that can be viewed as a substance, and he is also a tablet that is written upon. One aspect is as valid and as justifiable as another; but it is a cardinal mistake to confuse the two views.

The mason builds in granite and roofs with slate. The child learning its letters knows nothing of the qualities of its slate, but puzzles whether to write *c* or *k*. The mineralogist gives no precedence to one of the stones over the other; each has a constitution, a structure, properties, and uses. The educator ignores the granite; but, though he uses the slate, he does not therefore rate it higher, or deny the serviceability of the other material; he takes his substance as he finds it. His problem is whether the child should begins with words or letters; at what age, for what hours, in what sequence, and under what conditions, its education toward literacy should commence. To decide these issues upon crystallogical evidence because his pupils write upon a variety of stone would be as futile as if the geologist were to employ his knowledge of rocks for inferences as to the soundest principles of pedagogy.

So, if the student of human achievement were to try to withdraw from the observation of the natural historian and the mechanical philosopher the human beings upon whom is inscribed the civilization which he himself investigates, he would be ridiculous. And when on the other hand, the biologist proposes to rewrite history, in whole or in part, through the medium of heredity, he reveals himself in no more favorable light. True, he has the sanction of much precedent. But that an act has been done a thousand times, and is encouraged by the plaudits of tens of thousands, may make it safe, convenient, and harmless, or even profitable to the performer; it cannot render an inherently impossible attempt possible, nor convert a useless deed into a serviceable one.

There have been many attempts to make precise the distinction between instinct and civilization, between the organic and the social, between animal and man. Man as the clothing animal, the fire-using animal, the tool-using or tool-making animal, the speaking animal, are all summations that contain some approximation. But for the conception of the discrimination that is at once most complete and most compact, we must go back, as for the first precise expression of so many of the ideas with which we operate, to the uniquely marvelous mind that impelled Aristotle. "Man is a political animal." The word political has changed in import. We use instead the Latin term social. This, both philosopher and philologist tell us, is what the great Greek would have said were he speaking in English today. Man is a social animal, then; a social organism. He has organic constitution; but he has also civilization. To ignore one element is as short-sighted as to overlook the other; to convert one into the other, if each has its reality, is negation. With this basic formulation more than two thousand years old, and known to all the generations, there is something puny, as well as obstinately destructive, in the endeavor to abrogate the distinction, or to hinder its completest fruition. The attempt today to treat the social as organic, to understand civilization as heredity, is as essentially narrow minded as the alleged mediaeval inclination to withdraw man from the realm of nature and from the ken of the scientist because he was believed to possess an immaterial soul.

But, unfortunately, the denial, and for every denial a dozen confusions, still persist. They pervade the popular mind; and thence they rise, again and again, into the thoughts of avowed and recognized science. It seems, even, that in a hundred years we have retrograded. A century and two centuries ago, with a generous impulse, the leaders of thought devoted their energies, and the leaders of men their lives, to the cause that all men are equal. With all that this idea involves, and with its correctness, we need not here concern ourselves; but it certainly implied the proposition of equality of racial capacity. Possibly our ancestors were able to maintain this liberal stand because its full practical imports did not yet face them. But, whatever the reason, we have certainly gone back, in America and in Europe and in their colonies, in our application of the assumption; and we have receded too in our theoretic analysis of the evidence. Hereditary racial differences of ability pass as approved doctrine, in many quarters. There are men of eminent learning who would be surprised to know that serious doubts were held in the matter.

And yet, it must be maintained that not a single piece of evidence has yet been produced to support the assumption that the differences which one nation shows from another—let alone the superiority of one people to another—are racially inherent, that is organically founded. It does not matter how distinguished the minds are that have held such differences to be hereditary—they have only taken their conviction for granted. The historian can, and occasionally does, turn the case inside out with equal justification; and he then sees every event, every inequality, the whole course of human history, confirming his thesis that the distinctions between one group of men and another, past and present, are due to social influences and not in any measure to organic causes. Real proof, to be sure, is as wanting on one side as on the other. Experiment, under conditions that would yield satisfying evidence, would be difficult, costly, and perhaps contrary to law. A repetition of Akbar's interesting trial, or some modification of it, intelligently directed and followed out, would yield results of the greatest value; but it would not be tolerated by any civilized government.

There have been some attempts to investigate so-called racial distinctions with the apparatus of experimental psychology. The results incline clearly toward confirmation of the doctrine of the non-existence of organic differences. But too much stress may not as yet be laid on this conclusion, because what such investigations have above all revealed is that social agencies are so tremendously influential on every one of us that it is very difficult to find any test that, if distinctive racial faculties were inborn, would fairly reveal the degree to which they are inborn.

It is also well to remember that the problem of whether the human races are or are not in themselves identical, has innumerable practical bearings, which relate to conditions of life and to views that have intense emotional relations, so that an impartially abstract predisposition is rather rarely to be encountered. It is practically futile, for instance, to even touch upon the question with an American from the Southern states, or one tinged by Southern influences, no matter what his education or standing in the world. The actual social cleavage which is fundamental to all life in the South, and which is conceived of mainly as a race question, is so overshadowing and inevitable, that it compels, for the individual almost as firmly as for his group, a certain line of action, an unalterable and conscious course of conduct; and it could not well be otherwise than that opinions which flagrantly clash with all one's activities and with all the associated ideals, should arouse hostility. It is then but natural if the Southerner frequently receives the profession of racial equality, when it can be made to carry the conviction of sincerity to him, as an affront; and that he often meets even the most abstract, impersonal, and judicial consideration of the issues involved, with resentment, or, where this is checked by courtesy, with internal dissatisfaction.

The attitude of the Englishman in India, of the continental European in his colonies, is perhaps less extremely manifested; but all accounts indicate that it is no less settled.

On the other hand, the avowed and thoroughgoing Socialist or Internationalist must take the opposite stand, however unsympathetic it may be to him personally, or renounce all the aspirations that he holds dear. His inclination therefore, if generally less clearly defined, is no less predetermined and persistent.

Impartiality is thus not to be expected in this great case, except in some measure on the part of really detached and therefore uninfluential students; so that the maximum of assertion and rancor, and minimum of evidence, which prevail, are to be accepted as regrettable indeed, but as unavoidable and scarcely to be censured.

The problem being in the present state of our knowledge unprovable, is really also not arguable. What is possible, however, is to realize that a complete and consistent explanation can be given, for all so-called racial differences, on a basis of purely civilizational and non-organic causes; and to attain also to the recognition that the mere fact of the world in general assuming that such differences between one people and another are inborn and ineradicable except by breeding, is no evidence whatever in favor of the assumption being true.

The final argument, that one can actually *see* such national peculiarities born into each generation, and that it is unnecessary to verify the assumption because its truth is obvious to every one, has the least weight of all. It is of a kind with the contention that might be made that this planet is after all the fixed central point of the cosmic system because everyone can see for himself that the sun and stars move and that our earth stands still. The champions of the Copernican doctrine had this in their favor: they dealt with phenomena to which exactitude was readily applicable, about which verifiable or disprovable predictions could be made, which an explanation either fitted or did not fit. In the domain of human history this is not possible, or has not yet been found possible; so that an equal neatness of demonstration, a definitiveness of proof, a close tallying of theory with the facts to the exclusion of all rival theories, is not to be hoped for at present. But there is almost as fundamental a shifting of mental and emotional point of view, as absolute a turning upside down of attitude involved when the current thought of today is asked to view civilization as a wholly non-organic affair, as when the Copernican doctrine challenged the prior conviction of the world.

Most ethnologists, at any rate, are convinced that the overwhelming mass of historical and miscalled racial facts that are now attributed to obscure organic causes, or at most are in dispute, will ultimately be viewed by everyone as social and as intelligible only in their social relations. That there may be a residuum in which hereditary influences have been operative, it would be dogmatic to deny; but even this residuum of organic agencies will perhaps be found to be operative in quite other manners than those which are customarily adduced at present.

The opinion may further be uncompromisingly maintained, that for the historian—him who wishes to understand any sort of social phenomena—it is an unavoidable necessity, today, to disregard the organic as such and to deal only with the social. For the larger number who are not professional students of civilization, insistence upon these articles would be an unreasonable demand, under our present inability to substantiate them by proof. On the other hand, the social as something distinct from the organic is an old enough concept, and is a plain enough phenomenon about us in daily life, to warrant the claim that it cannot be outright dispensed with. It is perhaps too much to expect any one wedded, deliberately or unknowingly, to organic explanations, to discard these wholly before such incomplete evidence as is available to the contrary of these explanations. But it does seem justifiable to stand unhesitatingly on the proposition that civilization and heredity are two things that operate in entirely separate ways; that therefore any outright substitution of one for the other in the explanation of human group phenomena is crass; and that the refusal to recognize at least the logical possibility of an explanation of human achievement totally different from the prevailing tendency toward a biological one, is an act of illiberality. When once such recognition, of the rationality of this attitude of mind which is diametrically opposed to the current one, shall have become general, far more progress will have been made on the road towards a useful agreement as to the truth, than by any present attempts to win converts by argument.

One of the minds endowed with as eminent power of perception and formulation as any of our generation, Gustave Le Bon, whose name ranks high even if his regardless fearlessness has gained him but little of an avowed following, has carried the interpretation of the social as organic to its consistent consequence. His *Psychology of Peoples* is an attempt to explain civilization on the basis of race. Le Bon is really a historian of extraordinarily keen sensitiveness and perspicacity. But his professed attempt to resolve the civilizational materials with which he deals, directly into organic factors, leads him on the one hand to renounce his skilful interpretations of history until only intermittent flashes remain; and on the other hand, to rest his professed solutions ultimately on such mystic essences as the "soul of a race." As a scientific concept or tool, a race soul is as intangible and useless as any phrase of mediaeval philosophy, and on a par with Le Bon's ready declaration that the individual is to the race as the cell is to the body. If instead of soul of the race, the distinguished Frenchman had said spirit of civilization, or tendency or character of culture, his pronouncements would have commanded less appeal, because seeming vaguer; but he would not have had to rest his entire thought upon a supernatural idea antagonistic to the body of science to which he was trying to attach his work; and if non-mechanistic, his efforts at explanation would at least have earned the respect of historians.

As a matter of fact, Le Bon clearly operates with social phenomena, however insistently he gives them organic names and proclaims that he has resolved them organically. That "not the 18 Brumaire but the soul of his race established Napoleon," is biologically, and under any aspect of the science that deals with mechanical causality, a perfectly meaningless statement; but it becomes excellent history as soon as for "race" we substitute "civilization," and of course take "soul" in a metaphorical sense.

When he says that "cross breeding destroys an ancient civilization" he affirms only what many a biologist would be ready to maintain. When he adds: "because it destroys the soul of the people that possesses it," he gives a reason that must inspire any true scientist with a shudder. But if we change "cross breeding," that is, the mixture of sharply differentiated organic types, into "sudden contact or conflict of ideals," that is, mixture of sharply differentiated social types, the profound effect of such an event is indisputable.

Again, Le Bon asserts that the effect of environment is great on new races, on races forming through cross breeding of peoples of contrary heredities; and that in ancient races solidly established by heredity the effect of environment is nearly nil. It is obvious that in an old and firm civilization the actively changing effect of geographical environment must be small because the civilization has long since had ample opportunity to utilize the environment for its needs; but that on the other hand when the civilization is new—whether because of its transportation, because of its proceeding fusion from several elements, or from mere internal development—the renewing of relationship between itself and the surrounding physical geography must go on at a rapid rate. Here again good history is turned into bad science by a confusion that seems almost deliberately perverse.

A people is guided far more by its dead than by its living, Le Bon says. He is trying to establish the importance of heredity on national careers. What, though unrecognized by himself, lies at the bottom of his thought, is the truth that every civilization rests in the past, that however much its ancient elements are no longer living as such, they nevertheless form its trunk and body, around which the live sap-wood of the day is only a shell and a surface. That imposed education, a formal and conscious thing, can not give the substance of a new or another civilization to a people, is a verity that Le Bon has seized with vigor. But when he deduces this maxim as an inference from the unbridgeable abyss that eternally exists between races, he rests an obvious fact, which no person of discrimination has yet disputed, upon a mystical assertion.

It might nearly have been foreseen, after the above citations, that Le Bon would lay the "character" of his "races" to "accumulation by heredity." It has already been shown that if there is anything that heredity does not do, it is to accumulate. If, on the other hand, there is any one method by which civilization may be defined as operating, it is precisely that of accumulation. We add the power of flight, the understanding of the mechanism of the aeroplane, to our previous accomplishments and knowledges. The bird does not; he has given up his legs and toes for wings. It may be true that the bird is on the whole a higher organism than his reptilian ancestor, that he has traveled farther on the road of development. But his advance has been achieved by a transmutation of qualities, a conversion of organs and faculties, not by an increasing summation of them.

The whole theory of heredity by acquirement rests upon the confusion of these two so diverse processes, that of heredity and that of civilization. It has been nourished, perhaps, by unsatisfied needs of biological science, but it has never obtained the slightest unchallengeable verification from biology, and has in fact long been assailed, by a sound and vigorous instinct, as well as in consequence of the failure of observation and experiment, from within that science. It is a doctrine that is the constant blazon of the dilettante who knows something of both history and life, but has no care to understand the workings of either. Le Bon's studies being an attempt to explain one by the other, his utilization, sooner or later, of the doctrine of heredity by acquisition or accumulation, could almost have been predicted.

From a different and less aggressive temperament springs the wail that Lester Ward has voiced for a wide and aspiringly earnest element. Heredity by acquirement must take place, he argues, or there would be no hope of permanent progress for humanity. To believe that what we have gained will not be at least in part implanted in our children, removes the incentive to effort. All the labor bestowed upon the youth of the world would be in vain. Mental qualities are not subject to natural selection; hence they must be accumulated in man by acquirement and fixed by heredity. This view may be heard again and again from people who have arrived at the attitude through their own reflections, who have probably never read Ward directly or indirectly, and whose world seems to crash when its foundation of heredity is shaken. It is, if not a deep view, a common one; and for that reason Ward's formulation is, however worthless intrinsically, representative and significant. It reveals the tenacity, the insistence, with which many conscientious intellects of the day will not and can not see the social except through the glass of the organic. That this habit of mind can itself be depressing, that it forever prelimits development and eternally chains the future to the poverties and paucities of the present, does not dawn upon its devotees; is in fact probably the fixity which gives it its emotional hold.

It would seem probable that the greatest of the champions of acquired heredity, Herbert Spencer, was led to his stand by a similar motive. The precise method by which organic evolution takes place is after all essentially a biological problem, and not a philosophical one. Spencer, however, like Comte, was a sociologist as much as a philosopher. That he should have contested so stubbornly what in itself is a technical question of biology, is hardly intelligible except on the supposition that he felt the question to bear vitally on his principles; and that, in spite of his happy coinage of the term which has been prefixed as a title to the present essay, he did not adequately conceive of human society as holding a specific content that is non-organic.

When R. R. Marett, in opening his *Anthropology*—one of the most stimulating books produced in this field—defines the science as "the whole history of man as fired and pervaded by the idea of evolution," and adds that "anthropology is the child of Darwin—Darwinism makes it possible," he is unfortunately depicting the actual condition of this science with some truth; but as a program or an ideal his delineation must be challenged. Anthropology may be biology; it may be history; it may be an attempt to ascertain the relations of the two; but as history, the study of the social, shot through with the idea of organic evolution, it would be a jumble of diverse methods, and therefore no science in any sense of the term.

Of all the comminglings of the cultural with the vital, that which has crystallized under the name of the eugenics movement is the most widely known and of directest appeal. As a constructive program for national progress, eugenics is a confusion of the purposes to breed better men and to give men better ideals; an organic device to attain the social; a biological short cut to a moral end. It contains the inherent impossibility of all short cuts. It is more refined but no less vain than the short cut which the savage follows, when, to avoid the trouble and danger of killing his foe in the body, he pierces, in safety and amid objurgations uttered in the convenience of his own home, a miniature image addressed by the name of the enemy. Past ages have had their dragons of superstition to fight. Our battles against this ever re-arising brood dawn no smaller and as unceasing; and it would be shallow to try to defer or soften the inevitable conflict by withholding from this movement its true designation. Eugenics, so far as it is more than an endeavor at social hygiene in a new field, is a fallacy. It is a mirage like the philosopher's stone, the elixir of life, the ring of Solomon, or the material efficacy of prayer; and to those who are led by its learned modernity to receive it earnestly, it is a destructive snare. There is little to argue about it. If social phenomena are only organic, eugenics is right, and there is nothing more to be said. If the social is something more than the organic, eugenics is only an error and unclear thought; at whose childlikeness the future will smile, and then pass on.

Galton, the founder of the eugenics propaganda, was one of the most truly imaginative intellects produced by his country. Pearson, its distinguished living protagonist with scientific weapons, possesses one of the keenest minds of the generation. Hundreds of men of ability and eminence have professed themselves converts. It is plain that a simple fallacy must have presented itself in an envelope of enticing complications to be acceptable to them. Such men have not confounded important things that are intrinsically distinct, without a good reason. The explanation that Galton, Pearson, and the majority of the most creative of their followers were professional biologists, and therefore inclined to see the world through the lenses of the organic, is insufficient. Mere interest in one factor does not lead thinking minds practically to deny the existence of other factors. What then is the reason of the confusion into which they have precipitated themselves?

The cause seems to be a failure to distinguish between the social and the mental. All civilization in a sense exists only in the mind. Gunpowder, textile arts, machinery, laws, telephones are not themselves transmitted from man to man nor from generation to generation, at least not permanently. It is the perception, the knowledge and understanding of them, their *ideas* in the Platonic sense, that are passed along. Everything social can have existence only through mentality. Of course, civilization is not mental action itself; it is carried by men, without being in them. But its relation to mind, its absolute rooting in human faculty, is more than plain.

What, then, has occurred is that biology, which correlates and often identifies the physical and the mental, has gone one natural but as yet unjustified step further, and assumed the social as mental; whence the explanation of civilization in physiological and mechanical terms was an unavoidable consequence.

Now, the identification by modern science of the physical and mental is certainly correct. That is, it is justifiable as a method which can be consistently employed toward a coherent explanation of phenomena, and which leads to intellectually satisfactory and practically useful results. The correlation of the two sets of phenomena is made, or admitted, by all psychologists; it clearly holds for all faculties and instincts; and it has some definite physiological and chemical corroboration, though of a more crude and less completely established kind than is sometimes imagined. At any rate, this correlation is an unchallenged axiom of those who concern themselves with science: all mental equipment and all mental activity have an organic basis. And that is sufficient for present purposes.

This inseparability of physical and mental must be true also in the field of heredity. It is well known that where instincts are definite or specialized, as in insects, they are inherited as absolutely as are organs or structure. It is a matter of common experience that our own mental traits vary as much and as frequently tally with those of ancestors, as physical features. There is no logical reason, and nothing in the observation of daily life, that operates against the belief that an irascible temper is as heritable as the red hair with which it is traditionally associated, and that certain forms of musical aptitude may be as wholly congenital as blue eyes.

Of course there is much false inference in these matters, as regards man, through the interpretation of accomplishment as evidence of the degree of faculty. The discrimination of the two is not always easy; it frequently requires painstakingly acquired knowledge of facts, as well as careful judgment; and popular reasoning is likely to be scant of both. A powerful congenital faculty may establish the father successfully in a pursuit. This in turn may give an environmental influence, or a deliberate training, that will elevate the mediocre son, so far as his attainments are concerned, far above what his unaided natural faculties would have secured for him, and above many another individual of greater inherent capacities. The earning of a million is normally an indication of ability; but it normally requires intenser ability to earn a million after starting with nothing than to begin with a million received as a gift and increase it to three. That a musician is more frequently the son of a musician than not, at least when relative numbers are taken into account, is in itself no evidence at all that musical talent is heritable, for we know of purely social influences, such as Hindu caste, which attain similar results with far greater regularity than any one can assert heredity plus social influences to bring about among ourselves.

But it would be as unreasonable to exaggerate this caution into an outright denial of mental heredity, as to disregard it entirely.

There is then nothing in an off-hand survey of the situation to lead to a disbelief, and a large body of common experience to confirm the conviction, that characters of mind are subject to heredity much like traits of the body.

In addition, there is some proof, which, although not extensive, is hard to resist. Galton, in a fairly large series of records, has found the amount of regression—a quantitative index of the potency of heredity—to be the same for artistic faculty as for bodily stature. In another work he has investigated the blood relatives of eminent men, with the finding that eminence occurs among them with a frequency and in a degree exactly like the influence of heredity in respect to physical characters. Pearson has ascertained that the correlation—the degree of resemblance, quantitatively expressed, of phenomena available in numbers—between brothers is substantially the same for conscientiousness as for the shape of the head, for intellectual ability as for hair color, and so forth for other mental or moral and physical qualities. There is of course the possibility that in the data that underlie these results, as well as Galton's, there has been some confounding of temper with bad manners, of native intelligence with training of the intellect, of congenital artistic faculty with cultivated taste. But the attention of those who made the records seems to have been pretty definitely even though perhaps not always consciously directed to innate individual traits. Further, all the figures for the inheritance of these psychic characteristics agree as closely as could be expected with the corresponding ones relating to bodily features. The case may therefore be fairly regarded as substantially proved, at least until new evidence is available.

In spite of a wide acceptance of these demonstrations, especially by those predisposed to sympathize with biological progress, they have also met with some opposition, and with more ignoring than their bearing on a question of general interest warranted. In part this negative attitude may be due to a persistence of religious beliefs, in the main already superseded but not yet defunct, that center around the old concept of the soul, and which see in every linkage of mind and body an effacement of the cherished distinction of body and soul. But this belated conservatism will not account for all the failure of the Galton-Pearson demonstrations to meet universal acceptance or arouse wide enthusiasms.

The remainder of the opposition has been caused by Galton, Pearson, and their adherents themselves, who have not confined themselves to their well-supported conclusions, but have pressed on to further inferences that rest only on assertion. That heredity operates in the domain of mind as well as that of the body, is one thing; that therefore heredity is the mainspring of civilization, is an entirely different proposition, without any necessary connection, and certainly without any established connection, with the former conclusion. To maintain both doctrines, the second as a necessary corollary of the first, has been the habit of the biological school; and the consequence has been that those whose intellectual inclinations were otherwise, or who followed another method of research, have avowedly or tacitly rejected both propositions.

The reason why mental heredity has nothing to do with civilization, is that civilization is not mental action but a body or stream of products of mental exercise. Mental activity, as biologists have dealt with it, being organic, any demonstration concerning it consequently proves nothing whatever as to social events. Mentality relates to the individual. The social or cultural, on the other hand, is in its very essence non-individual. Civilization, as such, begins only where the individual ends; and whoever does not in some measure perceive this fact, though as a brute and rootless one, can find no meaning in civilization, and history for him must be only a wearying jumble, or an opportunity for the exercise of art.

All biology necessarily has this entire reference to the individual. A social mind is as meaningless a nonentity as a social body. There can be only one kind of organicness: the organic on another plane would no longer be organic. The Darwinian doctrine relates, it is true, to the race; but the race, except as an abstraction, is only a collection of individuals; and the bases of this doctrine, heredity, variation, and competition, deal with the relation of individual to individual, from individual, and against individual. The whole key of the success of the Mendelian methods of studying heredity lies in isolating traits and isolating individuals.

But a thousand individuals do not make a society. They are the potential basis of a society; but they do not themselves cause it; and they are also the basis of a thousand other potential societies.

The findings of biology as to heredity, mental and physical alike, may then, in fact must be, accepted without reservation. But that therefore civilization can be understood by psychological analysis, or explained by observations or experiments in heredity, or, to revert to a concrete example, that the destiny of nations can be predicted from an analysis of the organic constitution of their members, assumes that society is merely a collection of individuals; that civilization is only an aggregate of psychic activities and not also an entity beyond them; in short, that the social can be wholly resolved into the mental as it is thought this resolves into the physical.

It is accordingly in this point of the tempting leap from the individually mental to the culturally social which presupposes but does not contain mentality, that the source of the distracting transferences of the organic into the social is to be sought. A more exact examination of the relation of the two is therefore desirable.

In a brilliant essay written, under Pearsonian influence, on heredity in twins, Thorndike arrives anew, and by a convincing use of statistical evidence, at the conclusion that so far as the individual is concerned heredity is everything and environment nothing; that the success of our path in life is essentially determined at birth; that the problem of whether each one of us shall outstrip his fellows or lag behind them, is settled when the parental germ cells unite, and already long closed when the child emerges from the womb, all our careers run under the light of the sun being nothing but an unwinding, longer or shorter according to accident beyond our control, of the thread rolled on the spool before the beginning of our existence.

This finding is not only thoroughly elucidated by the author, but has the support of our common experience in life. No one can deny some measure of truth to the proverbial sow's ear that cannot be made into a silk purse. Every one numbers among his acquaintance individuals of energy, of address and skill, of what seems an uncanny prescience, or of a strength of character, that leave no doubts in our judgment that whatever their lot of birth, they would have risen above their fellows and been marked men and women. And on the other hand, we also admit regretfully the maladroit and sluggish, the incompetent and commonplace, who, born in any station, would have been of the mediocrities or unfortunates of their time and class. That Napoleon, set in another land and era, would not have conquered a continent, is sufficiently certain. The contrary affirmation may with fairness, it seems, be said to evince an absence of understanding of history. But the belief that under other circumstances this eternal beacon flame might have remained a household lamp, that his forces would never have been called forth, that a slight change of the accidents of epoch, place, or surroundings might have left him a prosperous and contented peasant, a shopkeeper or a bureaucrat, a routine captain retired on a pension—to maintain this argues a lack or a perverted suppression of knowledge of human nature. It is important to realize that congenital differences can have no effect on the course of civilization. But it is equally important to realize that we may and must concede the existence of such differences and their inextinguishability.

According to a saying that is also almost proverbial, and true to the degree that such commonplaces can be true, the modern schoolboy knows more than Aristotle; but this fact, if a thousand times so, does not in the least endow him with a fraction of the intellect of the great Greek. Socially—because knowledge must be a social circumstance—it is knowledge, and not the greater development of one individual or another, that counts; just as, to measure the true force of the greatness of the person, the psychologist or genetist disregards the state of general enlightenment, the varying degree of civilizational development, to make his comparisons. A hundred Aristotles among our cave-dwelling ancestors would have been Aristotles in their birthright no less; but they would have contributed far less to the advance of science than a dozen plodding mediocrities in the twentieth century. A super-Archimedes in the ice age would have invented neither firearms nor the telegraph. Bach born in the Congo instead of Saxony could have composed not even a fragment of choral or sonata, though we can be equally confident that he would have outshone his compatriots in some manner of music. Whether or not a Bach ever had birth in Africa, is another question—one to which a negative answer cannot be given merely because no Bach has ever appeared there; a question that in fairness we must admit to be unanswered, but in regard to which the student of civilization, until demonstration has been made, can make but one reply and pursue only one course: to assume, not as an end but as a condition of method, that there have been such individuals; that genius and ability occur with regular frequency, and that all races or large enough groups of men average alike and the same in qualities.

These are extreme cases, whose clearness is little likely to arouse opposition. Normally, the difference between individuals is less imposing, the types of society more similar, and the two elements involved are separable only by the exercise of some discrimination. It is then that the endless confusions begin. But if the factor of society and that of natal personality are distinct in the glaring examples, they are at least distinguishable in the more subtly shaded and intricate ones; provided only we wish to keep them apart.

If this is true, it follows that all so-called inventors of appliances or discoverers of thoughts of note were unusually able men, endowed from before birth with superior faculties, which the psychologist can hope to analyze and define, the physiologist to correlate with functions of organs, and the genetic biologist to investigate in their hereditary origins until he attains not only system and law but verifiable power of prediction. And, on the other hand, the content of the invention or discovery springs in no way from the make-up of the great man, or that of his ancestors, but is a product purely of the civilization into which he with millions of others is born as a meaningless and regularly recurring event. Whether he in his person becomes inventor, explorer, imitator, or user, is an affair of forces that the science of mechanical causality is concerned with. Whether his invention is that of the cannon or the bow, his achievement a musical scale or a system of harmony, his formulation that of the soul or that of the categorical imperative, is not explainable by the medium of mechanistic science—at least, not by any methods at the command of biological science—but finds its meaning only in such operations with the material of civilization as history is occupied with.

Darwin, whose name has been cited so frequently in the preceding pages, provides a beautiful exemplification of these principles. To deny this great man genius, mental eminence, inherent superiority to the mass of the human herd, would be fatuous. In Galton's famous classification, he would attain, by universal consent, at least to grade G, by an overwhelming majority no doubt to the still higher—the highest—grade X. That is, he was an individual born with capacities such as but fourteen, or more likely one, or still fewer, persons in every million possess. In short, he would have towered above his fellows in any and every society.

On the other side, no one can sanely believe that the distinction of Darwin's greatest accomplishment, the formulation of the doctrine of evolution by natural selection, would now stand to his credit had he been born fifty years sooner or later. If later, he would have been infallibly anticipated by Wallace, for one thing; by others, if an early death had cut off Wallace. That his giant restless mind would have evolved something noteworthy is as likely as it is a fact away from the point: the distinction of the particular discovery which he did make, would not have been his. Put on earth, by contrary supposition, a half century earlier, his central idea would not have come to him; as it failed to come to his brilliant predecessor, the evolutionist Lamarck. Or, it would have risen in his own mind, as it did in all its essentials in that of Aristotle, only to be at once discarded as logically possible indeed, but as unworthy of actual consideration. Or, finally, the thought might indeed have germinated and grown in him, but been ignored and forgotten by the world, a mere unfruitful accident, until European civilization was prepared, a few decades later, and hungry as well as prepared, to use it—when its rediscovery and not its barren formal discovery would have been the event of historical significance. That this last possibility is no mere idle conjecture is evidenced by its actually taking place in the case of one of the greatest of Darwin's contemporaries, his then unknown brother in arms, Gregor Mendel.

It is inconceivable that the independent occurrence of the idea of selection as the motive force of organic evolution, synchronously in the minds of Darwin and Wallace, should have been an affair of pure chance. The immediate acceptance of the idea by the world, proves nothing as to the intrinsic truth of the concept; but it does establish the readiness of the world, that is, of the civilization of the time, for the doctrine. And if civilization was prepared and hungry for the doctrine, the enunciation seems to have been destined to come almost precisely when it did come. Darwin carried with himself the germ of the idea of natural selection for twenty long years before he dared put forward the hypothesis which previously he had felt would be received with hostility, and which he must have thought insufficiently armed. It was only the briefer expression of the same insight by Wallace that led Darwin to publicity. Can it be imagined, if Wallace had met death at sea among the Malay islands, and Darwin, unspurred by his competitor colleague's activity, had carried his theory in hesitant privacy a few years longer and then suddenly succumbed to mortal illness, that we of the civilized world of today should have lived all our intellectual lives without a definite mechanism for evolution and therefore without any active employment of the evolutionary idea—that our biologists would be still standing where Linnaeus, Cuvier, or at most Lamarck stood? If so, the great currents of history would be absolutely conditioned by the lodgement or dislodgement of a bacillus in a particular human frame on a certain day; which conviction would certify to as much understanding as we should credit to him, who, finding in the high Andes the ultimate source of the tiny streamlet farthest removed in tortuous miles from the Atlantic ocean, should set his foot in the bubbling spring and believe that so long as he held it there the Amazon ceased to drain a continent and to pour its tide into the sea.

No. Wallace's crowding on Darwin's heels so that his too was a share, though a minor one, of the glory of the discovery, evidences that behind him trod still others, unnamed and perhaps forever themselves unconscious; and that had the leader or his second fallen by one of the innumerable accidents to which individuals are subject, the followers, one or several or many, would have pressed forward, would have been pressed forward, it would be better to say, and done their work—immediately, as history reckons time.

The total failure of Mendel's revolutionizing experiments in exact heredity to achieve the least recognition during their author's life, and for years after, has already been alluded to as an instance of the inexorable fate in store for the discoverer who anticipates his time. He is fortunate indeed if he is permitted to live out his lot in obscurity; and to escape the crucifixion which seemed a meet punishment for the first circumnavigator of Africa who saw the sun on his north. It has been said that Mendel's essay, in which are contained all the vital principles of the science that now bears his name, was published in a remote and little known source, and therefore failed for a generation to come to the notice of biologists. The last assertion may be challenged as unproved and inherently improbable. It is far more likely that biologist after biologist saw the essay, that some even read it, but that, one and all, it remained meaningless to them—not because they were unusually stupid men, but because they lacked the transcendent superiority of the occasional individual to see issues that lie ahead of those with which the world of their day is wrestling. Slowly, however, time rolled on and unconsciously a change of content of thought was preparing. Darwin himself had been concerned with the origin and nature of variations. When the first shock of overpowering novelty of his central discovery had begun to be assimilated by scientific conscience, this variation question trended to the front. The investigations of De Vries and Bateson, though their recognized outcome seemed only a destructive analysis of one of the pillars of Darwinism, were accumulating knowledge as to the actual operation of heredity. And then suddenly in 1900, with dramatic eclat, three students, independently and "within a few weeks of each other," discovered the discovery of Mendel, confirmed its conclusions with experience of their own, and a new science was launched on a career of splendid fulfillment.

There may be those who see in these pulsing events only a meaningless play of capricious fortuitousness; but there will be others to whom they reveal a glimpse of a great and inspiring inevitability which rises as far above the accidents of personality as the march of the heavens transcends the wavering contacts of random footprints on clods of earth. Wipe out the perception of De Vries, Correns, and Tschermak, and it is yet certain that before another year had rolled around, the principles of Mendelian heredity would have been proclaimed to an according world, and by six rather than three discerning minds. That Mendel lived in the nineteenth century instead of the twentieth, and published in 1865, is a fact that proved of the greatest and perhaps regrettable influence on his personal fortunes. As a matter of history, his life and discovery are of no more moment, except as a foreshadowing anticipation, than the billions of woes and gratifications, of peaceful citizen lives or bloody deaths, that have been the fate of men. Mendelian heredity does not date from 1865. It was discovered in 1900 because it could have been discovered only then, and because it infallibly must have been discovered then.

The whole history of inventions is one endless chain of parallel instances. An examination of patent office records, in any other than a commercial or anecdotic spirit, would alone reveal the inexorable order that prevails in the advance of civilization. The right to the monopoly of the manufacture of the telephone was long in litigation; the ultimate decision rested on an interval of hours between the recording of concurrent descriptions by Alexander Bell and Elisha Gray. Though it is part of our vulgar thinking to dismiss such conflicts as evidences only of unscrupulous cupidity and legal inadequacy or as melodramatic coincidences, it behooves the historian to see beyond such childlike plays of the intellect.

The discovery of oxygen is credited to both Priestley and Scheele; its liquefaction to Cailletet as well as to Pictet, whose results were attained in the same month of 1877 and announced in one session. Kant as well as La Place can lay claim to the promulgation of the nebular hypothesis. Neptune was predicted by Adams and by Leverrier; the computation of the one, and the publication of that of the other, had precedence by a few months.

For the invention of the steamboat, glory is claimed by their countrymen or partisans for Fulton, Jouffroy, Rumsey, Stevens, Symmington, and others; of the telegraph, for Steinheil and Morse; in photography Talbot was the rival of Daguerre and Niepce. The doubly flanged rail devised by Stevens was reinvented by Vignolet. Aluminum was first practically reduced by the processes of Hall, Héroult, and Cowles. Leibnitz in 1684 as well as Newton in 1687 formulated calculus. Anaesthetics, both ether and nitrous oxide, were discovered in 1845 and 1846, by no less than four men of one nationality. So independent were their achievements, so similar even in details and so closely contemporaneous, that polemics, lawsuits, and political agitation ensued for years, and there was not one of the four but whose career was embittered, if not ruined, by the animosities arising from the indistinguishability of the priority. Even the south pole, never before trodden by the foot of human beings, was at last reached twice in one summer.

A volume could be written, with but few years' toil, filled with endlessly repeating but ever new accumulation of such instances. When we cease to look upon invention or discovery as some mysterious inherent faculty of individual minds which are randomly dropped in space and time by fate; when we center our attention on the plainer relation of one such advancing step to the others; when, in short, interest shifts from individually biographic elements, which can be only dramatically artistic, didactically moralizing, or psychologically interpretable, and attaches whole heartedly to the social, evidence on this point will be infinite in quantity, and the presence of a majestic order pervading civilization will be irresistibly evident.

Knowing the civilization of an age and a land, we can then substantially affirm that its distinctive discoveries, in this or that field of activity, were not directly contingent upon the personality of the actual inventors that graced the period, but would have been made without them; and that, conversely, had the great illuminating minds of other centuries and climates been born in the civilization referred to, instead of their own, its first achievements would have fallen to their lot. Ericsson or Galvani eight thousand years ago would have polished or bored the first stone; and in turn the hand and mind whose operation set in inception the neolithic age of human culture, would, if held in its infancy in unchanging catalepsy from that time until today, now be devising wireless telephones and nitrogen extractors.

Some reservations must be admitted to this principle. It is far from established, rather the contrary, that extraordinary ability, however equal in intensity, is identical in direction. It is highly unlikely that Beethoven put in Newton's cradle would have worked out calculus, or the latter have given the symphony its final form. We can and evidently must admit congenital faculties that are fairly specialized. Everything shows that the elementary mental faculties, such as memory, interest, and abstraction, are by nature very uneven in individuals of equivalent ability but distinctive bent; and this in spite of cultivation. The educator who proclaimed his ability to convert a native memory for absolute numbers or for mathematical formulas into an equally strong retention of single tones or of complex melodies, would be distrusted as a charlatan or a fanatic. But it does not essentially matter if the originating faculty is one or several in mind. If Eli Whitney could not have formulated the difference between the subjective and the objective and Kant in his place would have failed to devise a practical cotton gin, Watt or Fulton or Morse or Stephenson could, in the place of the former, have accomplished his achievement, and Aristotle or Aquinas the task of the latter. It is possibly not even quite accurate to maintain that the individualities of the unknown inventor or inventors of the bow and arrow and those of firearms could have been interchanged, for the first production of the bow necessarily involved mechanical and even manual faculty, while the discovery of gunpowder and of its applicability to weapons may have required the different ability to perceive certain peculiar qualities of a more highly dynamic or chemical nature.

In short, it is a debatable point, though one of the greatest psychological interest, how far human faculty is divisible and subdivisible into distinct kinds. But the matter is not vital in the present connection, for there will hardly be any one rash enough to maintain that there exist as many distinguishable faculties as there are separate human beings; which in fact would be to assert that abilities do not differ in intensity or degree but only in direction or kind; in short, that while no two men were alike, all were absolutely equal in potential capacity. If this view is not correct, then it matters little whether the kinds of ability are several or many, because in any case they will be very few compared with the endless number of human organisms; because there will accordingly be so many individuals possessing each faculty, that every age must contain persons with low and mediocre and high measure of intensity of each; and the extraordinary men of one sort in one period will therefore still be substitutable for those of another time in the manner indicated.

If, therefore, any one's interpretation of mentality is disturbed by some of the particular equivalences that have been suggested, he can easily find others that seem more just, without dissenting from the underlying principle that the march of history, or as it is current custom to name it, the progress of civilization, is independent of the birth of particular personalities; since these, apparently averaging alike, both as regards genius and normality, at all times and places, furnish the same substratum for the social.

Here, then, we have an interpretation which allows to the individual, and through him to heredity, all that the science of the organic can legitimately claim on the strength of its actual accomplishments; and which also yields the fullest scope to the social in its own distinctive field. The accomplishment of the individual measured against other individuals depends, if not wholly then mainly, on his organic constitution as compounded by his heredity. The accomplishments of a group, relative to other groups, are uninfluenced by heredity because sufficiently large groups average alike in organic make-up.

This identity of average is incontestable for some instances of the same nations in closely successive ages—as Athens in 550 and 450, or Germany in 1800 and 1900—during which brief periods their hereditary composition could not possibly have altered to even a small fraction of the degree in which cultural achievement varied; it is certainly probably even for people of the same blood separated by long intervals of time and wide divergences of civilization; and it is, while neither proved nor disproved, likely to be substantially true, as suggested before, for the most distant races.

The difference between the accomplishments of one group of men and those of another group is therefore of another order from the difference between the faculties of one person and another. It is through this distinction that one of the essential qualities of the nature of the social is to be found.

The physiological and the mental are bonded as aspects of the same thing, one resolvable into the other; the social is, directly considered, irresolvable into the mental. That it exists only after mentality of a certain kind is in action, has led to confusion of the two, and even to their identification. The error of this identification is a fault that tends to pervade modern thinking about civilization, and which must be overcome by self-discipline before our understanding of this order of phenomena that fill and color our lives can become either clear or serviceable.

If the relation of the individual to culture here outlined is a true one, a conflicting view sometimes held and already alluded to, is unentertainable. This view is the opinion that all personalities are, while not identical, potentially equal in capacity, their varying degrees of accomplishment being due solely to different measures of accord with the social environment with which they are in touch. This view has perhaps been rarely formulated as a generic principle; but it seems to underlie, though usually vaguely and by implication only, many tendencies toward social and educational reform, and is therefore likely to find formal enunciation at some time.

This assumption, which would certainly be of extensive practical application if it could be verified, seems to rest ultimately upon a dim but profound perception of the influence of civilization. More complete that this influence of civilization is upon national fortunes than upon individual careers, it nevertheless must influence these latter also. Mohammedanism—a social phenomenon—in stifling the imitative possibilities of the pictorial and plastic arts, has obviously affected the civilization of many peoples; but it must also have altered the careers of many persons born in three continents during a thousand years. Special talents which these men and women possessed for delineative representation may have been suppressed without equal compensation in other directions, in those whose endowment was unique. Of such individuals it is undoubtedly true that the social forces to which they were subject depressed each of them from successful attainment to more mediocre. And without question the same environment elevated many an individual to high rank above his fellows whose special abilities, in some other age and country, would have been repressed to his private disadvantage. The personality born with those qualities that lead to highly successful leadership of religious brigands, for instance, is undoubtedly assured of a more prosperous and contented career in Morocco than in Holland of today.

Even within one nationally limited sphere of civilization, similar results are necessarily bound to occur constantly. The natural logician or administrator born into a caste of fishermen or street sweepers is not likely to achieve the satisfaction in life, and certainly not the success, that would have been his lot had his parents been Brahmins or Kshatriyas; and what is true formally of India holds substantially for Europe.

But, that a social enviroment may somewhat affect the fortunes and career of the individual as measured against other individuals, does not prove that the individual is wholly the product of circumstances outside of himself, any more than it means that the opposite is true and that a civilization is only the sum total of the products of a group of organically shaped minds. The concrete effect of each individual upon civilization is determined by civilization itself. Civilization appears even in some cases and in some measure to influence the effect of the individual's native activities upon himself. But to proceed from these realizations to the inference that all the degree and quality of accomplishment by the individual is the result of his moulding by the society that encompasses him, is pure assumption, extreme at that, and directly at variance with all observation, both as immediate apperception and as it survives critical analysis.

Therefore it is possible to hold to the historical or civilizational interpretation of social phenomena without proceeding to occupy the position that the human beings that are the given channels through which civilization courses, are only and wholly the products of its stream. Because culture rests on specific human faculty, it does not follow that this faculty, the thing in man that is supra-animal, is of social determination. The line between the social and the organic may not be randomly or hastily drawn. The threshold between the endowment that renders the flow and continuance of civilization possible and that which prohibits even its inception, is the demarcation—doubtful enough once, in all probability, but gaping for a longer period than our knowledge covers—between man and animal. The separation between the social itself, however, the entity that we call civilization, and the non-social, the pre-social or organic, is the diversity of quality or order or nature which exists between animal and man conjointly on the one hand, and the products of the interactions of human beings on the other. In the previous pages the mental has already been subtracted from the social and added to the physically organic which is subject to the influence of heredity. In the same way it is necessary to eliminate the factor of individual capacity from the consideration of civilization. But this elimination means its transfer to the group of organically conceivable phenomena, not its denial. In fact nothing is further from the path of a just prosecution of the understanding of history than such a negation of differences of degree of the faculties of individual men.

In short, social science, if we may take that word as equivalent to history, does not deny individuality any more than it denies the individual. It does refuse to deal with either individuality or individuals. And it bases this refusal solely on its denial of the validity of either factor for the achievement of its proper aims.

It is true that historical events can also be viewed mechanically, and expressed ultimately in terms of physics and chemistry. Genius may prove definable in unit characters or the constitution of chromosomes, and its particular achievements in osmotic reactions of nerve cells. The day may come when what took place in the tissue of Darwin's brain when he first thought the concept of natural selection, can be profitably studied, or even approximately ascertained, by the physiologist and chemist. Such an achievement, shockingly destructive as it may seem to those whom revelation appals, would be not only defensible, but worth while—of enormous interest, even, and possibly of the highest utility. Only, it would not be history; nor a step toward history or social science.

To know the precise reactions in Darwin's nervous system at the moment when the thought of natural selection flashed upon him in 1838, would involve a very genuine triumph of science. But it would mean nothing historically, since history is concerned with the relation of doctrines such as that of natural selection to other concepts and social phenomena, and not at all with the relation of Darwin himself to social phenomena or other phenomena. This is not the current view of history; but, on the other hand, the current view rests upon the endlessly recurring but obviously illogical assumption that because without individuals civilization could not exist, civilization therefore is only a sum total of the psychic operations of a mass of individuals.

There are those, of mechanistic proclivities and interests, who hold that it is only when historical events are explained on a basis like that assumed in our example, that history will have any significance. They have pressed their view, sometimes by assertion, more often by implication, until it has come to be widely accepted. But it is true only if a single method of thought is the sole one to be accorded validity and justifiability. If the ability to weigh the moon renders Shelley's poetry a useless superfluity, well and good: there is nothing more to be said. There actually are people fanatic enough to take such a stand. But if scientific methods give science, and artistic exercise yields literature, and the two do not exclude each other because they do not come into conflict and are not even comparable; if the justification of each is in its results and not in any toleration extended by the other; and if the truly unforgivable sin with the crown of futility is not to practise one without regard to the other but to try to practise one by means of the other;—then, too, it is at least conceivable that there may be a third activity, neither science nor art in their strict senses, but history, the understanding of the social, which also has an aim that cannot be denied and whose justification must be sought in its own results and not by the standard of any other activity. That is all that history as an intellectual manifestation can ask; but that it must ask. Mechanistic science has accomplished wonders in a brief space by adhering ever more rigidly to its own peculiar methods, and allowing no limits to be set to its application of these methods. Yet that a tool has proved its service for a purpose, does not affect the value of other purposes or the utility of other tools for these other purposes. Whenever the mechanistic scientist proposes to accomplish all the work of the human understanding by his science, he is taking the stand either of his predecessor of ruder days who mingled the objects and means of science and theology, or science and art, or science and morals; or of that other often cited but perhaps imaginary predecessor who, when spiritual power made him arrogant, denied the right of existence of science as a goal or science as a method;

proclaiming that divine truth was the only and sufficient truth. Intolerance is ever born anew. But science has escaped from alleged persecution, and then from timid servility, only so recently, that it should remember not to attempt oppression and negation in its turn.

History, then, justifies itself in proportion that it is mechanistically "unscientific"; that it has its own method, its own equivalent to the causality of science; and, in one sense, its own material. Not that there is a range of subjects that can be delimited and assigned respectively to science and to history. In fact, the applicability of science to any and all domains of human cognizance must be expressly affirmed. But the same phenomenon can after all be viewed with different ends. The social is scientifically resolvable; but it is resolvable through the individual—the organic and psychic individual. History deals with the social by resolving it into the social without the medium of the individual.

No!

Science will attack historical material—social material—by converting it into organic terms—whether psychical or physical does not matter, so long as the ever present individual physiological aspect or basis of the social phenomena is dealt with. These organic results will then be ready for interpretation by the methods of physics and chemistry. Thus the material will be made part of that great unit, the system that justifies and elevates science to its high plane—the system that is pervaded by the principle of mechanical causality as its essence. But history, without denying this principle, without concerning itself with it or with the methods that flow from it, keeps its intent fixed upon the unaltered and irresolved facts of the social plane, upon historical data apperceived and utilized directly.

!

As, then, there are two lines of intellectual endeavor in history and in science, each with its separate aim and set of methods; and as it is only the confounding of the two that results in sterile negation; so also two wholly disparate evolutions must be recognized: that of the substance which we call organic and that of the other substance called social. Social evolution is without antecedents in the beginnings of organic evolution. It commences late in the development of life—long after vertebrates, after mammals, after the primates even, are established. Its exact point of origin we do not know, and perhaps shall never know; but we can limit the range within which it falls. This origin occurred in a series of organic forms more advanced, in general mental faculty, than the gorilla, and much less developed than the first known race that is unanimously accepted as having been human, the man of Neandertal and Le Moustier. In point of time, these first carriers of the rudiments of civilization must antedate the Neandertal race by far, but must be much posterior to other extinct human ancestors of the approximate intellectual level of the modern gorilla and chimpanzee. Evidence fails, and in the present connection the determination would be of little moment.

The beginning of social evolution, of the civilization which is the subject of history, thus coincides with that mystery of the popular mind: the missing link. But the term "link" is misleading. It implies a continuous chain, a strand that is the same in texture before and beyond the break in knowledge. But with the unknown bearers of the primeval and gradually manifesting beginnings of civilization, there took place a profound alteration rather than an improved passing on of the existing. A new factor had arisen which was to work out its own independent consequences, slowly and of little apparent import at first, but gathering weight, and dignity, and influence; a factor that had passed beyond natural selection, that was no longer wholly dependent on any agency of organic evolution, and that, however rocked and swayed by the oscillations of the heredity that underlay it, nevertheless floated unimmersibly upon it.

The dawn of the social thus is not a link in any chain, not a step in a path, but a leap to another plane. It may be likened to the first occurrence of life in the hitherto lifeless universe, the hour when that one of infinite chemical combinations took place which put the organic into existence, and made it that from that moment on there should be two worlds in place of one. Atomic qualities and movements were not interfered with when that seemingly slight event took place; the majesty of the mechanical laws of the cosmos was not diminished; but something new was inextinguishably added to the history of this planet; as when a perpetually reeling thread becomes two from one.

Or, one might compare the inception of civilization to the end of the process of slowly heating water. The expansion of the liquid goes on a long time. Its alteration can be observed by the thermometer as well as in bulk, in its solvent power as well as in its internal agitation. But it remains water. Finally, however, the boiling point is attained. Steam is produced: the rate of enlargement of volume is increased a thousand fold; and in place of a glistening percolating fluid, a volatile gas diffuses invisibly. Neither the laws of physics nor those of chemistry are violated; nature is not set aside; but yet a saltation has taken place: the slow transitions that accumulated from zero to one hundred have been transcended in an instant, and a condition of substance with new properties and new possibilities of effect is in existence.

Such, in some manner, must have been the result of the appearance of this new thing, civilization. We need not consider that it abolished the course of development of life. It certainly has not in any measure done away with its own substratum of the organic. And there is no reason to believe that it was born full fledged. All these incidents and manners of the inception of the social are after all of little consequence to an understanding of its specific nature, and of the relation of that nature to the character of the organic substance that preceded it in absolute time and still supports it. The point is, there was an addition of something new in kind, an initiation of that which was to run a course of its own.

We may sketch the relation which exists between the evolutions of the organic and of the social (fig. 8). A line, progressing with the flow of time, rises slowly, but ever gatheringly. At a certain point, another line begins to diverge from it, insensibly at first, but ascending ever farther above it on its own course; until, at the moment where the curtain of the present cuts off our view, each is advancing, but far from the other, and uninfluenced by it.

In this illustration, the continuous line denotes the level inorganic; the broken line, the evolution of the organic; the line of dots, the development of civilization. Height above the base is degree of advancement, whether that be complexity, heterogeneity, degree of coördination, or anything else. A is the beginning of time on this earth as revealed to our understandings. B marks the point of the true missing link, of the first human precursor, the first animal that carried and accumulated tradition. C would denote the state reached by what we are accustomed to call primitive man, that Neandertal *homo* who was our forefather in culture if not in blood; and D, the present moment.

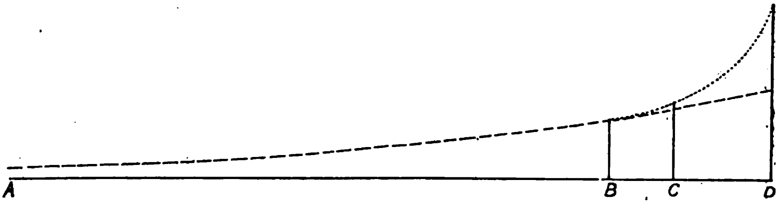

Fig. 8.

It is inevitable that if there is any foundation for the contentions that have been set forth, an arguing from one of these lines to the other must be futile. To assert, because the upper line has risen rapidly just before it is cut off, that the one below it must also have ascended proportionally more in this period than in any before, is obviously uncompelling. That our institutions, our knowledge, the exercising of our minds, have advanced dizzyingly in twenty thousand years is no reason that our bodies and brains, our mental equipment and its physiological basis, have advanced in any corresponding measure, as is sometimes argued by scientists and generally taken for granted by men at large. If anything, it might rather be an evidence that the lower, organic line has fallen off in its rate of ascent. The bodies and minds in this line have continued to carry civilization; but this civilization has met the struggle of the world in such a way that much of the stress has been directed from these bodies and minds. We do not argue that the progress of organic evolution is *prima facie* indication that inorganic matter is more complex, more advanced in its combinations, or in any sense "higher," than it was fifty million years ago; much less that organic evolution has taken place through an inorganic evolution as cause. And no more can we infer from social development to a progress of the hereditary forms of life.

In fact, not only is the correlation of the lines of organic and social development as unjustifiable theoretically as it would be to argue from the compressibility or weight of water to that of steam; but all the evidence known directs us to the conviction that in recent periods civilization has raced at a speed so far outstripping the pace of hereditary evolution, that the latter has, if not actually standing still, afforded all the seeming, relatively, of making no progress. There are a hundred elements of civilization where there was one in the time when the Neandertal skull enclosed a living brain; and not only the content of civilization but the complexity of its organization has increased a hundredfold. But the body and the associated mind of that early man have not, by any scale that can be applied, attained a point a hundred times, not even twice, as fine, as efficient, as delicate, or as strong, as they were then; it is doubtful if they have improved by a fifth. There are, it is true, those who make the contrary assertion. Yet it seems the fair-minded must avow that such assertions rest not on any objective interpretation of the facts, but on a wish to find a correlation, a desire to make the thread of evolution a single, unbranching one, to see the social only as organic.

Here, then, we have to come to our conclusion; and here we rest. The mind and the body are but facets of the same organic material or activity; the social substance—or unsubstantial fabric, if one prefers the phrase,—the existence that we call civilization, transcends them utterly for all its being forever rooted in life. The processes of civilizational activity are almost unknown to us. The self-sufficient factors that govern their workings are unresolved. The forces and principles of mechanistic science can indeed analyze our civilization; but in so doing they destroy its essence, and leave us without understanding of the very thing which we seek. The historian as yet can do little but picture. He traces and he connects what seems far removed; he balances; he integrates; but he does not really explain, nor does he transmute phenomena into something else. His method is not science; but neither can the scientist deal with historical material and leave it civilization, nor anything resembling civilization, nor convert it wholly into concepts of life and leave nothing else to be done. What we all are able to do is to realize this gap, to be impressed by its abyss with reverence and humility, and to go our paths on its respective sides without self-deluding attempts to bridge the eternal chasm, or empty boasts that its span is achieved.

UNIVERSITY OF CALIFORNIA

For God's sake - . . . .
Turn to pg: 441

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SOME ARCHAEOLOGICAL WORK IN PORTO RICO[1]

BY HERMAN K. HAEBERLIN

JUEGO DE BOLA

IN June 1915 the author excavated a so-called *juego de bola* or ball-court situated in the limestone mountains of the interior of Porto Rico several miles northwest of the town of Utuado. More precisely this site is located in the district called *Barrio Rio Arriba de Arecibo,* about a quarter of a mile north of the point where the municipal road which runs from the schoolhouse of this *barrio* to the *Barrio Santa Rosa* crosses the *Quebrada de los Medinas,* a little brook which flows into the *Rio Grande de Arecibo* from the west. The land in which the site is located, is owned by Señor Blas Gau, whose house stands several hundred feet south of the *juego.*

The site was one of those inclosures popularly known by the natives as ball-courts (*juego de bola*) because they believe that they were used by the Indians for playing ball.[2] About the location of these ball-courts Fewkes says the following:

> The ball courts examined by the present author were situated for the most part on terraces or on land fringing rivers, elevated high enough to be above freshets, and yet lying in river valleys that could be cultivated.[3]

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[1] The excavations reported in this paper were executed for the Natural History Survey of Porto Rico which is being conducted jointly by the New York Academy of Sciences and by the Government of Porto Rico. The anthropological division of this survey is in charge of Dr. Franz Boas under whose directions the fieldwork of this paper was performed. The specimens found are in the American Museum of Natural History.

I would like to express our indebtedness to Dr. Charles P. Berkey, of Columbia University, and to Mr. Gratacap, of the American Museum of Natural History, for furnishing the mineralogical and geological information published in this paper. Dr. Berkey and his assistants also analyzed the soil specimens and supplied the data concerning the texture of the pottery.

[2] See Fewkes, "The Aborigines of Porto Rico and Neighboring Islands," *Twenty-fifth Annual Report Bureau of American Ethnology,* p. 79.

[3] Fewkes, *op. cit.,* p. 82.

The location of the *juego de bola* excavated corresponds to this statement. It was situated at the bottom of a small valley which runs from north to south and through which the *Quebrada de los Medinas* flows. The level bottom of this valley was under cultivation. The inclosure was some fifty feet or more east of the brook, at the foot of the mountain which incloses the valley on the east.

Before excavation the only visible signs of an archaeological site consisted of two parallel rows of stones projecting out of the soil and partly overgrown by dense vegetation. These stones were irregular in size and shape but all were more or less long and flat. These stood on end and projected vertically out of the soil as seen in the photograph which was taken before work was begun (fig. 9).

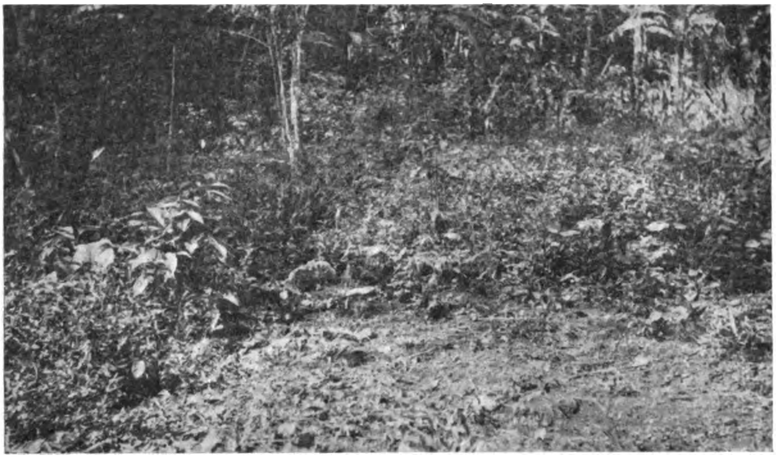

FIG. 9.

On the eastern third of the *juego* coffee trees were growing. On the remaining two-thirds beans, maize, and tobacco had been planted at different times.

As is shown schematically in the drawing (fig. 10) the *juego* was rectangular and bordered by two parallel rows of upright stones running from east to west. The row on the south was continuous, all the stones obviously standing in their original positions. In the northern row, however, there were considerable breaks near the middle of the line. In the western half of the inclosure a row of stones lying horizontally on the surface soil and with the flat side down crossed the *juego* in the way indicated in the drawing, thus forming a pavement. The horizontal position of these stones on the present surface of the soil and thus contrasting with the position of the other stones which stood vertically and were imbedded in the present surface, left no doubt that this row of stones was not in its original place. This was corroborated by the natives who remembered that these stones had been removed by them from the northern row of vertical stones in order to make a rude pavement over the wet and swampy soil of the valley. In the course of the excavation the stones of this pavement were replaced into the gaps mentioned above in the northern row of stones. The way in which they fitted into these gaps proved that the statement of the natives was correct.

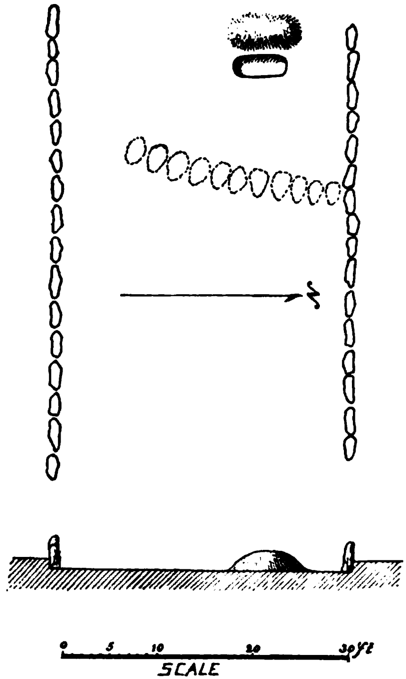

FIG. 10.

The two parallel rows of stones ran exactly from east to west. The rectangle they formed was open at the two shorter sides, that is to say on the east and on the west. There were no traces whatever of stones having stood at these two sides. The distance between the two parallel rows of stones varied from twenty-nine and a half to thirty-one feet. The northern row of stones was forty-four feet long, the southern one fifty feet, three inches.

The rows of stones were approximately straight. The deviations from the straight line were, I think, caused in the course of time by the dampness of the ground and the effect of cultivation. Almost all of the stones were more or less flat, but otherwise quite irregular in size and shape. They varied in length from twelve to thirty-nine inches. The largest slab was the last one in the southwestern corner. None of the stones showed any traces of workmanship whatsoever. If they ever existed, they have been eroded. The two rows of stones were continuous, the stones being set closely together. Most of the slabs were still standing upright. There can be no doubt that this was the original position of all of them. It is not surprising that some had fallen over in the course of time in the damp soil.

A trench dug lengthwise through the *juego* showed that two layers could be easily distinguished. A dark surface layer lay over one of yellow clay. The upper layer was carefully removed. All finds were deposited between these two layers. This proves that the surface of the lower layer was the original surface of the *juego*.

Excavation showed that the yellow layer was five to six inches deeper inside of the two rows of stones than outside of them. The profile of the *juego* after the dark upper layer had been removed is shown in the lower drawing of figure 10. The stones, as seen in this picture, were not buried in the original surface, that is to say in the lower layer, but were held in place by simply resting against the higher soil outside of the *juego*. The two rows of stones formed a low continuous wall on the northern and southern sides of the *juego*.

Within the *juego* the surface of the yellow layer, the original floor, was quite horizontal and level, with one exception however. After the removal of the dark surface layer the yellow layer proved to form a mound near the northwest corner of the *juego* (see fig. 10). This mound was one foot high, eight and one-half feet long from north to south and four feet wide from east to west. Directly east of this mound was a hole one foot deep, but of much smaller circumference. There were no deposits within the mound or near it that could give a clue as to its significance.

As we are quite in the dark as to the meaning of these Porto Rican sites, popularly called *juegos de bola*, we were careful to look for any signs of decoloration, which might have been interpreted as house-posts or the like. It is, however, possible to state definitely that there were no such decolorations either within the *juego* or at the base of the stones.

As already stated all the deposits found in the *juego* lay on the surface of the yellow layer. They consisted chiefly of potsherds. These were found in large numbers scattered over the site. Some were ornamented. The largest pieces lay near the base of the stones. This was very likely due to the protection the deposits found there.

All the potsherds found here belonged to one and the same type of dark red pottery. Figure 11 shows a number of samples. No traces of paint could be found on this pottery. The style of ornamentation on this pottery was uniform. The technique was incised work. The ornaments consisted either of scroll-work or of combinations of straight lines of which figure 11 shows several typical examples. The scroll-work of these sherds is of the same type as that on the pot of figure 28. This pot was found in a cave near the *juego*. It was deposited under guano and is one of the few intact pots from Porto Rico.

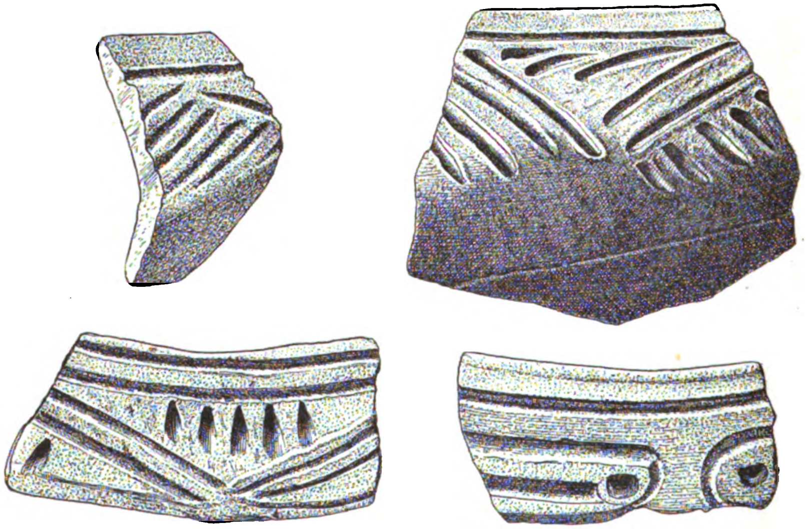

FIG. 11.—Potsherds. (25.0–1536.)

Besides the potsherds a number of green stones and a small piece of quartz (25.0–1549) were found in the *juego*, all of them between the two layers. One greenish-gray stone is about 12 cm. long and is a diorite porphyry. One edge may possibly show some flaking. None of the other stones show traces of workmanship. They are disintegrated and reconsolidated tuffs. One light green stone is according to Mr. Gratacap consolidated, redeposited volcanic sand. The fact that these stones were all found on the original surface of the *juego* makes it seem plausible that these stones may be rejects. Attention is called to the fact that the celts mentioned below (figs. 23 and 24) are also made of tuffs.

Near the *juego*, but not in it, the celt of figure 12 was found. It is made of coral. Its surface is rough and shows no polishing.

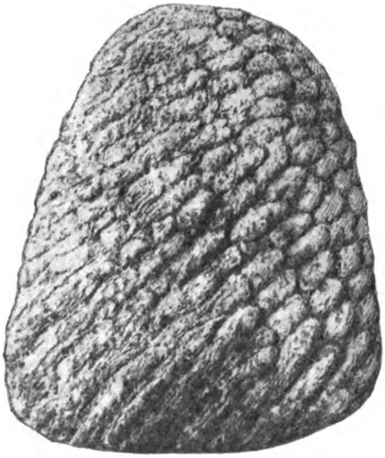

FIG. 12.—Celt. Actual length 8.5 cm. (25.0–1557.)

Near the center of the *juego* a spherical stone was discovered. It is about 9 cm. in diameter and does not appear to be an artifact. Apparently it is one of the stones of which Fewkes says:

> The discovery of stone balls in these inclosures is often mentioned as an indication that these places were used in ball games, implying that the stones were the balls used. . . . Indeed, some of the larger stone balls, which are more than 2 feet in diameter, could hardly be carried by a single man. Moreover, many of the balls are not spherical, but are simply waterworn bowlders having the form of oblate or prolate spheroids. Considering these facts I have serious doubt whether the stones could have been used in the kind of ball game described by Oviedo. . . .[1]

Obviously these doubts are well warranted. I do not venture to suggest what these stones may have been used for. A black stone ball (25.0–1552) was brought to me by some peasants who said they found it near the *juego*. It is nicely polished and fairly symmetrical. It is 5.5 cm. in diameter and is according to Mr. Gratacap a limonitic clay pebble.

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[1] Fewkes, *Twenty-fifth Annual Report Bureau of American Ethnology*, p. 84.

Several feet east of the *juego* lay a few tremendous bowlders, which had obviously fallen down the steep mountain-side. At the base of these rocks a number of sherds were found. They were of the same type as those from the *juego* itself. Some of them were ornamented.

Dr. Fewkes speaks of burial mounds near the *juegos* examined by him. He writes:

> Just outside the boundary wall of every one of the inclosures studied by the author there were found one or more low mounds which bear superficial evidences of having been made by human hands.[1]

He found that these mounds were used for burials. This leads him to an interpretation of the *juegos*. He writes:

> The conclusions drawn from my excavations of the Utuado mounds are that large numbers of the dead were buried just outside the dance courts and that the elaborate *areitos*, or mortuary dances, were held in the latter.[2]

Attention must be called to the fact that no traces of such mounds were found near the *juego* excavated by the present author.

CUEVA DE LA SEIBA

The limestone mountains of the interior of Porto Rico are literally honeycombed with caves of all sizes.[3] About one mile west of the *juego* which has just been described a cave was excavated. It is known locally as the *Cueva de la Seiba*. It is situated on the land of the *Hacienda Jobo* at the head of a little valley that runs from north to south. The entrance to this cave is about sixty feet above the bottom of the valley. The slope on which it opens is quite abrupt. The general appearance of the cave was that characteristic of limestone caves in general. Stalactites and stalagmites formed chambers, niches, and partitions.

The entrance was not large. Its breadth was eleven feet, its height at present only about four feet. The latter measurement however must have formerly been greater. Several bowlders about three feet high now block the entrance. It is evident from the circumstances that they fell down from the outer edge of the ceiling of the cave. The height of the entrance before these rocks became detached must have been about seven feet.

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[1] Fewkes, *ibid.*, p. 82.

[2] Fewkes, *ibid.*, p. 83.

[3] Fewkes, *ibid.*, p. 87.

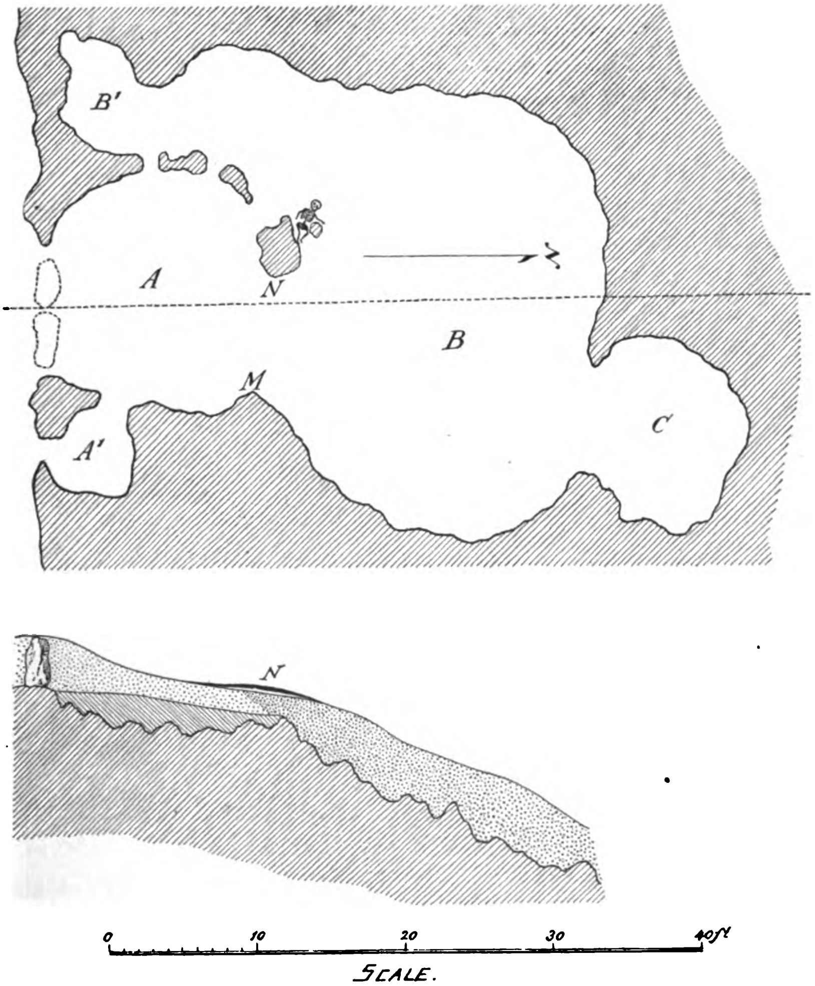

FIG. 13.—*Cueva de la Seiba.*

Figure 13 gives an idea in a schematic way of the general form of the cave with its subdivisions. The shaded areas indicate the rock formations. The cave can be roughly divided into three main chambers. The first one from the entrance marked *A* was clearly defined from the rest of the cave by a series of stalactites. It was approximately circular with a diameter of about sixteen feet. The greatest height of this chamber from the floor to the ceiling was about eight or nine feet before the soil of the floor had been removed. The entrance to the cave was at the south side of this chamber. A small and very low chamber (*A'*) led off from *A* on its east side. Between points *M* and *N* was the passageway to the second and largest chamber. As is seen in the drawing, this chamber surrounded chamber *A* on two sides. It measured twenty-two and one-half feet in depth from the point *N* northward. The floor of this chamber sloped down considerably towards the rear as seen in the cross-section. At its northernmost point it was five and one-half feet deeper than the floor of *A*. Chamber *B* extended very far around chamber *A* to the south. At *B'* it terminated in what might be regarded as a distinct chamber of small size. Finally a chamber *C*, much smaller than *B*, led off from the latter on the north. Its measurements were 9½ by 12½ feet. Its floor was about four feet deeper than the deepest point of chamber *B*.

The *Cueva de la Seiba* was very favorable for archaeological work, because it showed no traces of former digging. On the very densely populated island of Porto Rico few caves have escaped the hand of the random digger. The layers of this cave showed no indications of having been disturbed.

The first obvious traces of human activity consisted in a number of faces carved on the ends of stalactites. All of these rock carvings were on the northern and northwestern sides of chamber *A* and faced the entrance of the cave. Two of the faces were well preserved and casts were made of them. See figures 14 and 15. Traces of two or three other faces were discernible, but the constant action of the water was quickly making them disappear. It is surprising that the rock carvings survived as long as they did. It is impossible to say how many faces were originally carved on the stalactites of the cave.

Chamber *A* was the first to be excavated. In it two layers could be distinguished (see profile of fig. 13). The upper layer consisted of dark soil about one foot in depth at the center of the chamber. Under it was a homogeneous layer of light soil which extended down to the limestone formations of the floor of the cave. It covered all the irregularities of this floor. This light layer contained nothing that might be interpreted as the trace of human remains.

The general appearance of its soil (25.0–1533) was light yellowish and granular. It was nowhere decolored. Mineralogically it consisted of lime powder more or less stained with limonite (95 per cent.) and of siliceous spicules (5 per cent.). By origin it was disintegrated, Oligocene limestone. Of organic substance it contained some very small roots.

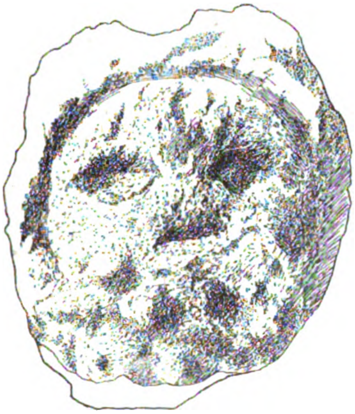

FIG. 14.—Stone carving 18.5 cm. high. (25.0–1563.)

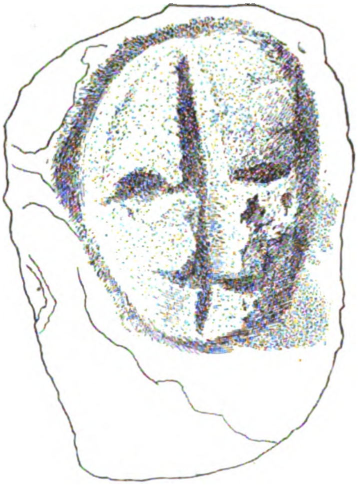

FIG. 15.—Stone carving 19.5 cm. high. (25.0–1564.)

In the center of the chamber this lower layer was about one and one-half feet deep. On the eastern side of chamber *A* the yellow layer was much higher than in the center and here the dark layer over it was very thin. At the narrow passageway to the small side-chamber *A′* the lime dripping down from the ceiling had formed a plate immediately over the yellow layer. This plate was removed.

About five inches below the surface of the yellow layer two marine shells were found at this point. In the little chamber *A'* the dark layer was missing altogether. Only the yellow soil was present. Nothing was found here excepting a tertiary sea-urchin.

The surface of the dark layer of chamber *A* sloped upwards towards the entrance of the cave and there covered the bowlders mentioned above. See profile of figure 13. A number of potsherds and shells of crabs were found in the dark layer of this chamber.

The soil of this upper layer (25.0–1532) was brownish gray, earthy, and somewhat granular in appearance. It was a mixture of weathered limestone, ashes, and calcined crab remains. It consisted of the following minerals: lime powder (80 per cent.), calcite crystals (10 per cent.), diatom spicules (?) (1 per cent.), fragments of charcoal (5 per cent.).

In the western corner of chamber *A* the prevailing dark soil was replaced by some soil of somewhat different texture and of considerably lighter color. It was light gray, earthy, lumpy in appearance. It consisted of lime powder (70 per cent.), lime powder stained with limonite (9 per cent.), crystals of calcite (10 per cent.), and fragments of charcoal (10 per cent.). Of organic matter chela and carapace of crab were conspicuous features.

In this soil was found a gray stone about 10 cm. long (25.0–1540). It is broken. Two of its surfaces are quite smooth and appear to be polished artificially. It is an andesyte according to Mr. Gratacap.

The floor of chamber *A* was more or less horizontal, but the surface of the soil in chamber *B* sloped down considerably. As is seen in the sketch, this slope began between the two points marked *M* and *N*. While two distinct layers were discernible in chamber *A*, there was only one such layer in chamber *B*. The texture of this layer was similar to that of the dark layer of chamber *A*. In general appearance it was, however, finer and much less brownish in color and more decidedly gray. The difference in color is indicated in the sketch by difference in shading. The transition from the top layer of *A* to the layer of *B* was more or less gradual. Near point *N* the layer consisted of lime powder (85 per cent.), calcite crystals (5 per cent.), and charcoal fragments (10 per cent.).

See sample 25.0–1530. In the rear of chamber *B* the percentage of charcoal increased, the comparison here being as follows: lime powder 85 per cent., fragments of charcoal 14 per cent., grains of quartz 1 per cent. See sample 25.0–1541. Special features of the soil of chamber *B* were gastropod fragments, mouth piece, chela, and carapace of crab.

Between the points *M* and *N* the gray layer was ten to twelve inches in depth. As is indicated in the cross-section near point *N* a more complicated stratification was observed. Over the thick gray layer two very thin layers could be distinguished. The upper one was only one-half inch thick and consisted of black earth; under it was another thin layer, about one inch in thickness and of different texture.

To summarize the results of the analysis of the soils from different parts of the cave, the important data are that the chief constituent of all the samples is disintegrated, Oligocene limestone and that all the soil of the cave excepting that of the lower layer of chamber *A* contains a high percentage of charcoal fragments and of crustacean remains. Evidently the lower layer of *A* alone antedates human activity in the cave. This is corroborated by the fact that none of the artifacts were found in this layer.

Numerous pieces of tuffs and andesites (25.0–1562) were found in the gray soil near the entrance to chamber *B* and farther back in this chamber. These stones are not indigenous to a limestone cave and therefore must have been brought in from the outside. They do not show traces of having been worked with the exception of one tuff which may be artificially polished.

Besides the small fragments of organic matter already mentioned in the analyses of the soils from the cave, the gray soil of chamber *B* abounded in enormous quantities of shells of snails, shells of crabs, bones of a rodent, and also some bird bones. The rodent bones proved to be of considerable zoological interest, inasmuch as they belong to a new genus and species. Dr. J. A. Allen of the American Museum of Natural History has published a description of them in the *Annals of the New York Academy of Sciences.*[1] He has given to this rodent the name: *Isolobodon portoricensis.* It is a large rodent allied to *Plagiodontia.*[1] Of the condition of the bones Dr. Allen writes:

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[1] *Annals of the New York Academy of Sciences*, vol. XXVII, pp. 17–22.

> They are highly coated with a gray ashy covering, easily removed with a soft brush, and have the appearance and general character of recent bones, having undergone no mineralization nor much discoloration.[2]

And finally in the conclusion he makes the following interesting remarks:

> *Isolobodon*, like *Plagiodontia*, is evidently of recent extinction. In the case of *Plagiodontia*, the only extant specimen, so far as known to me, is the type of the species in the Paris Museum of Natural History, described by F. Cuvier in 1836. . . . It is mentioned as being nocturnal and frugivorous, its flesh as very good to eat, and that for this reason "les Haitiens, qui en sont très friands, les recherchent si soigneusement, qu'ils ont fini par rendre ces animaux très rare." In all probability it was soon after completely exterminated. It is also probable that *Isolobodon* had already become extinct in the neighboring island of Porto Rico, doubtless from a similar cause, and perhaps not long prior to the discovery of the island by Europeans. At least the fresh condition of its remains found in Seiba Cave seems to imply recent extinction.

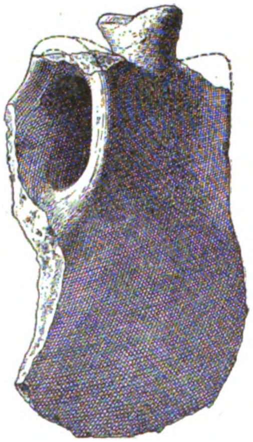

FIG. 16.—Potsherd. (25.0–1561.)

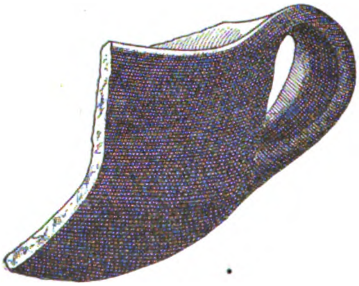

FIG. 17.—Potsherd. (25.0–1542.)

Considering the nature of the soil in which these bones were found, the great quantities in which they occurred, and the nature of the organic matter with which they were mixed, there can be little doubt that the rodent was actually used by the natives as food. Bird bones were found in the same stratum. They belong, according to Mr. W. De W. Miller of the American Museum, to a pigeon and a parrot. The shell-animals that were deposited in great quantities in the cave have been kindly identified by Mr. Miner of the American Museum. The following species were represented:

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[1] *Ibid.*, p. 17.

[2] *Ibid.*, p. 18.

Megalomastoma Croceum Gmelin (= *M. cylindraceum* Chemn.)
Helix (*Parthena*) *angulata* Fer.
Helix (*Caracolus*) *Carocolla* Linn.
Helix (*Thelidomus*) *lima* Fer.
Cistula rusei Pfr.
Subulina octona Brug.

In the soil of chamber *B* a large number of potsherds were found. A few also lay in the top layer of chamber *A*. Many of these sherds are large and show the shape of the original pot fairly well. Figures 16 and 17 show two large sherds. In the same way as all the sherds found in the *juego* represent one type of pottery, so do those from the cave also belong to one and the same type. But it is interesting that these two types are different. The pottery from the cave is much heavier and also darker in color. The differences in color and chemical composition are not due to secondary effects of the different soils in which the two kinds of pottery lay imbedded, but are inherent. Dr. Berkey kindly made photomicrographs of the two types, figures 18 and 19 (magnification about 30 diameters). These figures show the difference in texture very plainly. Dr. Berkey supplied the following information.

> The pottery from the cave (fig. 18) shows a coarser grain than the other, a greater variety of size of grain, a greater variety of composition of grain, a larger amount of dark material, a larger amount of clay filling or binding, and a more pronounced flowage structure, a structure that must have been developed in the modeling of the product.

Concerning the pottery from the *juego* Dr. Berkey says:

> The microscope shows a very great abundance of fragments of comparatively clear mineral matters constituting the bulk of the material. They are bound together by a clayey product (or what was once clay) and this matter seems to be

present in not much greater amount than must have been necessary to hold the multitude of fragments together. The structure is essentially massive. Hardly anywhere is there any flowage effect. The general makeup seems to indicate that considerable care was taken to select comparatively pure, angular sand as one of the constituents; and a comparatively pure, fine clay as the other. The excellent mixing of the two things, and the selection of material indicated would seem to me to indicate a higher type of workmanship and appreciation of differences in material than is indicated in the first type.

In contradistinction to the pottery of the *juego* that of the cave is almost altogether devoid of ornamentation. Incised lines which are so prominent a feature of the former are missing almost completely on the sherds from the cave. Only a very few sherds show simple straight incised lines. The typical scroll-work of the other type of pottery is absent entirely. These differences in the two types cannot be accidental on account of the large number of sherds found. Very wide handles are a prominent trait of the pottery from the cave (see figs. 16 and 17). The existence of two distinct types of pottery in Porto Rico is interesting and deserves further study. Fewkes seems to be under the impression that there is only one type.[1]

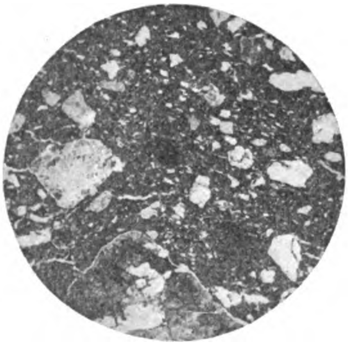

FIG. 18.—Photomicrograph of pottery from the cave.

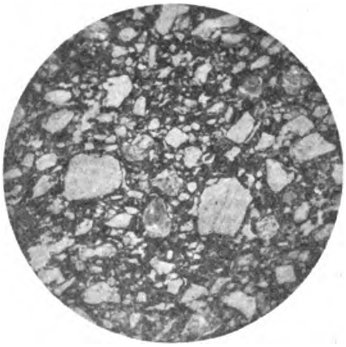

FIG. 19—Photomicrograph of pottery from the *juego*.

On the sherd of figure 16 there is a knob between the two shoulders of a large handle. It is made by the technique of modelling. The sherd of figure 35 shows a similar feature, however, with the difference that the knob and the two shoulders are elaborated into crude faces.

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[1] Fewkes, "The Aborigines of Porto Rico and Neighboring Islands," *Twenty-fifth Annual Report, Bureau of American Ethnology*, p. 180.

In the ashy material of chamber *B* a shell of a turtle was found, likewise a fragment of a light green polished celt (fig. 20). It is made of amphibolite. Dr. Berkey informed me that this is a rare stone in Porto Rico and that he never saw an amphibolite from this island with such a fine structure. Another unworked piece of amphibolite was found in the cave (25.0–1562). In the ashes of chamber *B* there was also a small polished pebble (25.0–1546). It is 3 cm. long and is an andesite-porphyry. In the small side chamber *B′* a number of rodent bones were dug up.

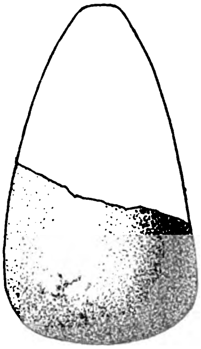

FIG. 20.—Light green polished celt. Natural size. (25.0–1534.)

Most of the deposits in chamber *B* were found near the sides adjourning chamber *A*. In the deepest and most northern part of the chamber only several sherds and the two round stones mentioned below were found.

Finally in the last chamber, *C*, were unearthed a number of potsherds and a heap of snail shells. The sherds are of the same type as those from the other chambers. Two of them have simple parallel incised lines near the rim of the pot. In this chamber was also found a dark green and very hard stone (25.0–1544). It fits comfortably into the hand. The two broad surfaces of this stone show a marked polish. Mr. Gratacap believes that this is doubtlessly artificial. It would seem that this stone was used for the purpose of rubbing. It is an aphanitic trap and must have been brought into the limestone cave.

From the side of the small and dark chamber, *C*, a narrow passageway, difficult of access, led around chamber *B* on the east towards *A′*. This passageway was entered, but in it no traces of archaeological remains were discovered.

In the ashes of chamber *B* two round pebbles, 7 and 7.5 cm. in diameter respectively, were found (25.0–1561). A similar stone lay in chamber *C* (25.0–1544). The surfaces of these three stones are quite smooth. Probably they were used for rubbing or for cooking purposes. The smoothness is apparently not natural. All three stones are serpentinized diorites according to Mr. Gratacap.

An interesting find was made in a small niche of the ceiling of the cave near the point *M*. This niche was formed by the grotesque shapes of the limestone. Here we found the pretty pendant of figure 21. It is 5.2 cm. in height. A portion of the one side is broken off. It is made of bone and the lines are scratched in.

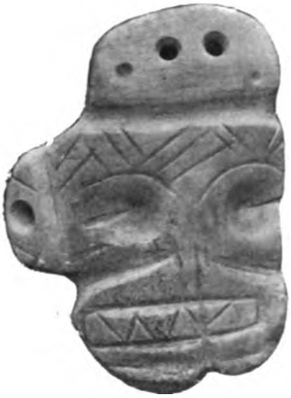

FIG. 21.—Bone pendant.

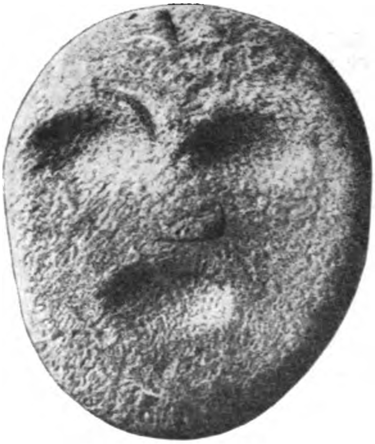

FIG. 22.—Facial carving in stone. 9.5 cm. x 11.3 cm. (25.0–1550.)

Immediately north of chamber *A* the skeleton of a very small baby was imbedded in the ashy soil. It lay between two limestone columns as shown in figure 5 with its head towards the west. The bones are well preserved, especially the skull.

As has been stated above, the soil of chambers *A* and *B* contained a good deal of charcoal. This led us to make a special effort to ascertain whether or not traces of a fireplace were present. Investigations showed that this was decidedly not the case.

MISCELLANEOUS SPECIMENS FROM THE SAME REGION

Near the house of Mr. Frederico Schellhorn, the manager of the *Haciendo Jobo*, and about a half a mile south from the site of the *juego* described above there had previously been the traces of another *juego*. These were still plainly visible several years ago, as Mr. Schellhorn informed me. But now the ground is under cultivation and the encircling stones have been removed. Several

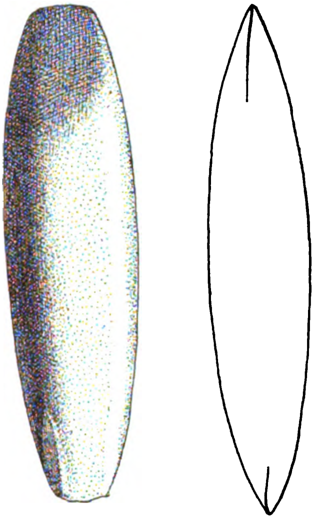

FIG. 23.—Celt of green stone 9.8 cm. long. (25.0–1555.)

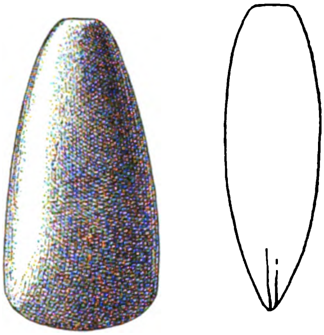

FIG. 24.—Celt of green stone 6.1 cm. long. (25.0–1556.)

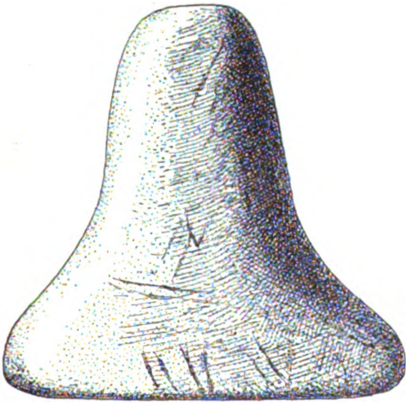

FIG. 25.—Stone implement 7.8 cm. long. (25.0–1551.)

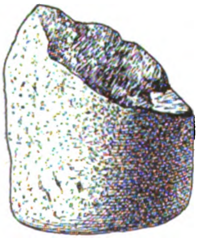

FIG. 26.—Stone fragment natural size. (25.0–1553.)

objects were brought to me which the peasants had found on this site while cultivating the land. The most interesting specimen obtained in this way was the flat stone with facial carving shown in figure 22. Similar objects are illustrated on Plate LV of Fewkes' monograph. The size of the above specimen is approximately the same as those shown by Fewkes. The two small green celts of figures 23 and 24 were found on the same site. They are andesitic tuffs according to Mr. Gratacap. The grayish-brown stone of

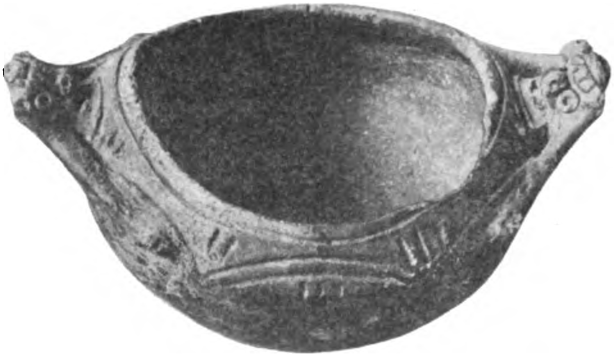

FIG. 27.—Pottery bowl 9 cm. high. (25.0–1567.)

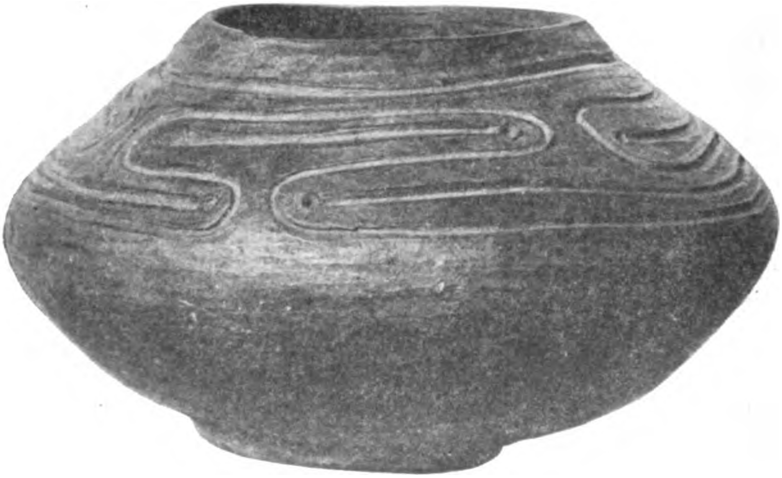

FIG. 28.—Pottery bowl 9.5 cm. high. (25.0–1537.)

figure 25 is also from this site. It is flat and its base is of long oval shape. This base is quite smooth and suggests that the specimen may have been an implement for pounding or rubbing. Finally the red stone of figure 26 was found on this destroyed *juego*. It is a red diorite, of regular cylindrical shape, and broken off at the one end. Probably it was a pestle of the type shown by Fewkes on Plates XXV–XXVII.

The pot in figure 28 was found and bought on the *Hacienda Jobo*. As I was told, it had been imbedded under guano in a cave on the right bank of the *Quebrada de Jobo*. Its shape and ornamentation is interesting. I would like to call especial attention to the rim around the bottom which is visible on the photograph. This rim serves as a foot on which the pot stands firmly.

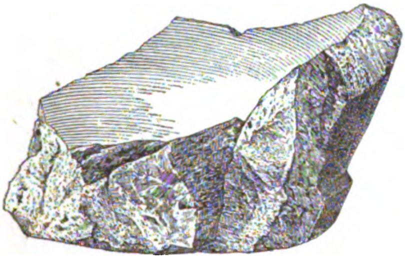

FIG. 29.—Fragment of mortar made of burnt clay, 20.5 cm. wide. (25.0–1426.)

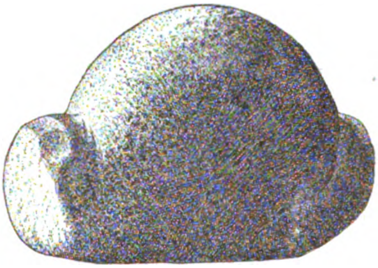

FIG. 30.—Triangular stone, 10.2 cm. high. (25.0–1480.)

APPENDIX

In the following I would like to enumerate and describe briefly a number of specimens which were purchased in Porto Rico by Dr. Alden Mason and Prof. Franz Boas. These objects are also in the American Museum of Natural History.

25.0–1567 (fig. 27). This is another intact bowl. It is 9 cm. in height and represents a type of red ware. It does not have a foot at its bottom as the pot of figure 28 does. Around the rim there is a band of simple incised work. The two handles are modelled into faces of a characteristically Porto Rican type.

25.0–1426 (fig. 29). A fragment of a mortar made of burnt clay. The inside is smooth and well rounded. As shown on the drawing a raised band ran around the base of the mortar.

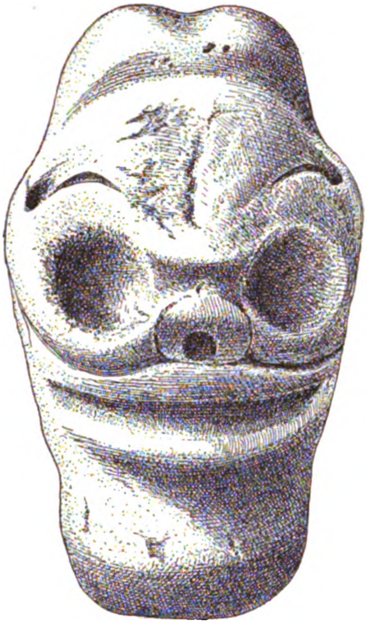

FIG. 31.—Triangular stone seen from above, 22 cm. long. (25.0–1467.)

25.0–1480 (fig. 30). One of the many triangular stones of the type so often reported from Porto Rico. Their use is enigmatical. The present specimen is quite plain in workmanship. It is a volcanic basalt. It is marked as having been found on the ball-court in Bayane.

25.0–1467. Another triangular stone. Figure 31 shows it from above. When seen from the side, the face is built up triangularly. The workmanship is excellent. Its surface is well smoothed. Presumably the stone is a fossil coral.

25.0–1477 (fig. 32). A small stone figure apparently representing an animal lying on its belly, but with its head twisted upward. The features of the face are marked by grooves. On the side

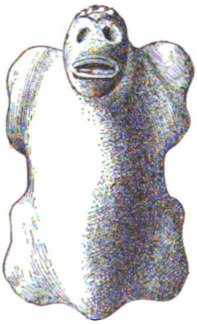

FIG. 32.—Stone figure representing an animal, 5.6 cm. high. (25.0–1477.)

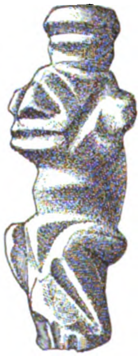

FIG. 33.—Stone figurine, 6.7 cm. high. (25.0–1529.)

opposite the face each foreleg is pierced through by a hole (not visible on drawing). These borings were evidently used for suspending the stone as a pendant of some kind.

25.0–1529 (fig. 33). A small figure of a being standing with bent knees or sitting on its haunches. The technique by which it is worked is interesting. The forms of the face and of the legs are shaped by means of long straight grooves. These must have been made by means of a filing-process.

The specimens of figures 32 and 33 are both made of white stone of essentially the same nature. Dr. Berkey defined this material as follows: "A metamorphic silicate rock made up chiefly of diopside and amphibole." It is of very fine grain. This material is native to Porto Rico in contact zones.

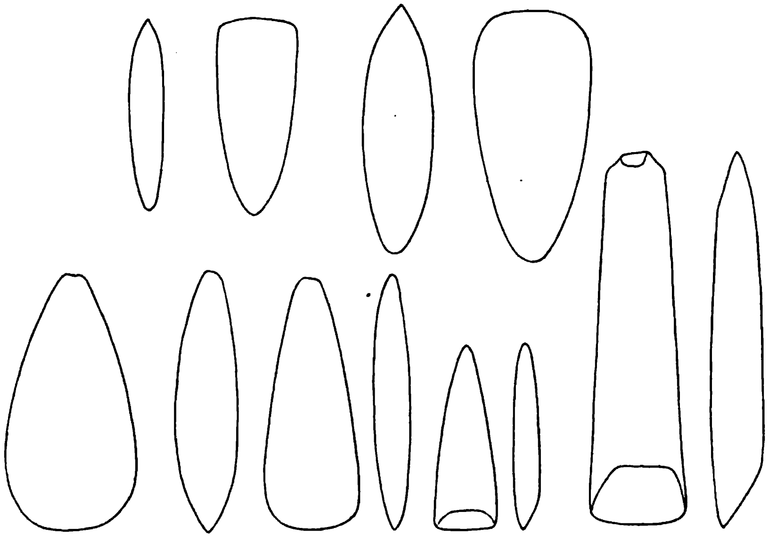

FIG. 34.—Outlines of celts. (Upper row, 25.0–1485, 25.0–1455. Lower row, 25.0–1454, 25.0–1456, 25.0–1464, 25.0–1463.)

Figure 34 shows the shapes and sizes of six different celts. These are fairly typical of the forms of all celts in the collection. Dr. Berkey determined their mineralogical origins as follows:

25.0–1454. Probably an indurated ash.

25.0–1455. A fine-grained andesite.

25.0–1456. An indurated volcanic ash.
25.0–1463. Probably an ash.
25.0–1464. A contact rock, metamorphosed limestone.
25.0–1485. An andesite, finely porphyritic with some flowage.

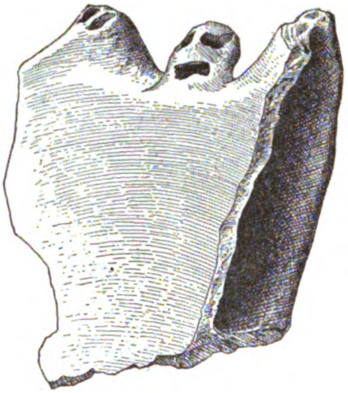

FIG. 35.—Potsherd, 12 cm. high. (25.0–1499.)

25.0–1499 (fig. 35). A potsherd with a broad handle of the same type as the handle of the sherd on figure 16. The specimen has already been referred to on page 228.

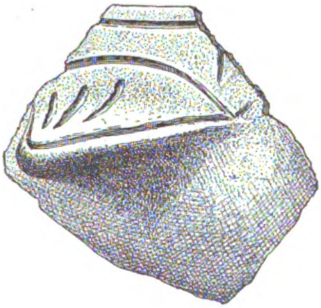

FIG. 36.—Potsherd with projecting handle, 6.2 cm. high. (25.0–1511.)

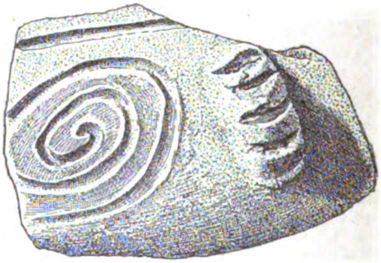

FIG. 37.—Potsherd with incised spiral, 7.8 cm. wide. (25.0–1423.)

25.0–1511 (fig. 36). A potsherd with a projecting handle which is ornamented by incised lines.

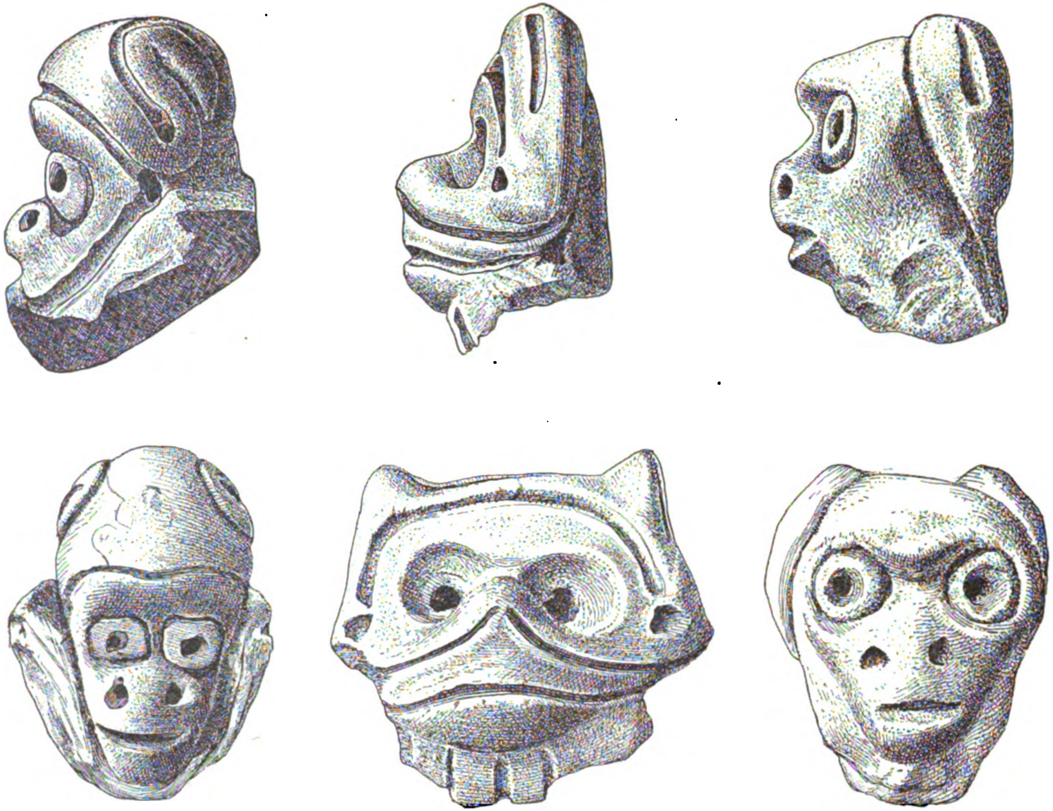

FIG. 38 —Three faces of burnt clay, front and side view. A, 5 cm. high; B, 4.5 cm. high; C, 4 cm. high. (25.0–1420.)

25.0–1423 (fig. 37). A potsherd with an incised spiral and an elevated knob of clay. This knob has transversal grooves.

25.0–1420, 25.0–1510 (figs. 38 and 39). Four faces of burnt clay. They are fairly representative of the large number of such faces purchased. Three of them are shown *en face* as well as in profile. Such faces are very well known from Porto Rico as well as from many other West Indian islands. They are broken off from pots to which they were originally attached, see figure 27. The technique of modelling these faces is very characteristic of West Indian art. The features are worked out by means of incised lines and holes. The eyes are typical. They are ordinarily made of a hole with a concentric line. The prognathous jaws make the faces look like those of monkeys. This is a frequent feature. 25.0–1510 (fig. 39) is different in many respects from the other faces. It is beautifully made and shows remarkable realism in its details.

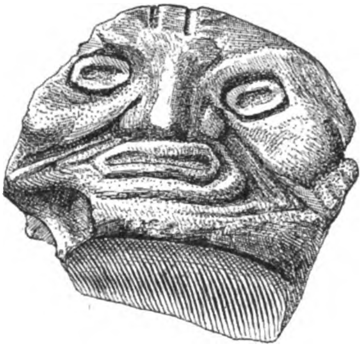

FIG. 39.—Face of burnt clay, 4.7 cm. wide. (25.0–1510.)

Archaeological Note.—Finally I would like to call attention to the following remark, for which I am indebted to the geological department of Columbia University, and which may interest some other archaeologist working in Porto Rico. In the valley of Jueyes river back of the Coamo limestone ridge there are Indian shell heaps intermixed with much pottery. In a cave of the ridge there are rock carvings. Natives living near this site stated that nobody had as yet visited this spot for archaeological work.

COLUMBIA UNIVERSITY,
NEW YORK CITY

FURTHER NOTES ON HUMAN REMAINS FROM VERO, FLORIDA

By E. H. SELLARDS

THE human remains from Vero, Florida, the discovery of which was announced in July, 1916, have been very fully discussed, and it may seem that a further communication is unnecessary. However, in the "Symposium" on this subject which appeared in the *Journal of Geology* (Jan.–Feb., 1917) there developed considerable diversity of opinion among the geologists and anthropologists who visited the locality, some of whom accept the evidence that the human remains are of Pleistocene age as conclusive, while others are not convinced.[1] Dr. Hrdlička is of the opinion that the human remains and artifacts represent recent inclusions by burial. Dr. Chamberlin, on the other hand, although accepting the human remains and artifacts as contemporaneous with these deposits, regards the associated Pleistocene fossils as secondary, assuming that they were originally imbedded in an older deposit and were subsequently removed and washed into the deposits in which they are now found. Dr. MacCurdy regards the human remains and artifacts as normal inclusions within these deposits, but is doubtful as to the Pleistocene age of the beds. Dr. Hay regards the human remains and associated fossils as contemporaneous and is of the opinion that the deposits may be as old as middle Pleistocene. Dr. Vaughan verifies the geologic section, but withholds judgment as to the age of the human remains awaiting the accumulation of additional evidence.

A description of the geologic section at Vero and also an account of the discovery of the human bones in association with the extinct vertebrates have been given in papers previously published and hence may be omitted from this paper. Accordingly at this time it will be necessary to consider only the objections that have been offered to the conclusion that the human remains and the associated fossils are contemporaneous. It is assumed also that reference may be made to the earlier papers on the Vero deposits for details not included in the present paper.[1]

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[1] "Symposium on the Age and Relations of the Fossil Human Remains found at Vero, Florida," E. H. Sellards, Rollin T. Chamberlin, Thomas Wayland Vaughan, Aleš Hrdlička, Oliver P. Hay and George Grant MacCurdy. *The Journal of Geology*, vol. xxv, 1917, pp. 1–62.

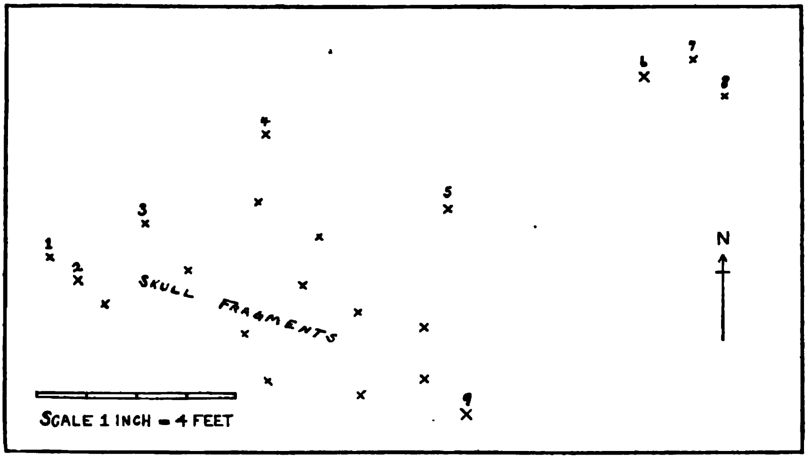

FIG. 40.—Ground plan showing the location of human bones found in the canal bank at Vero in April and in June, 1916. Index to bones; 1, left ulna; 2, a part of the shaft of the same bone; 3, left femur; 9, a part of the shaft of the same bone; 4, radius; 5, metatarsal; 6, astragalus; 7, external cuneiform; 8, part of ilium. *Courtesy of University of Chicago Press.*

DO THE HUMAN REMAINS AND ARTIFACTS REPRESENT BURIALS?

The question as to whether or not the human bones represent burials may perhaps be best discussed by considering the bones of the individual found in the south bank of the canal west of the lateral inlet. Of the bones of this individual, the right ulna, a part of the left humerus, a part of a scapula, one incisor, and parts of the skull had fallen from the bank. Of these bones the ulna, the humerus, and a piece of the frontal are bleached from exposure to the sun. The other bones mentioned were found in cavings which had recently fallen from the bank, and do not show bleaching. All of the other bones that have been obtained at this locality were found in place in the bank. The bones which apparently may safely be attributed to this individual include, in addition to those mentioned, the left ulna (in two pieces), the shaft of the right femur (in two pieces), the proximal part of the left radius, the ascending ramus of the right lower jaw, two metatarsals, numerous fragments of the skull and some pieces of ribs. Bones found a little farther to the east which may or may not pertain to this individual include a right astragalus, a right external cuneiform, a piece from the right pubes, a part of a left ilium, two phalanges, and a section from a limb bone, as well as some other bone fragments. These last named bones are from stratum no. 2 of the section, while all of the others listed were on the contact line of nos. 2 and 3.

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[1] *The American Journal of Science*, vol. 42, 1916, pp. 1–18; *Science*, vol. 44, Oct. 1916, pp. 615–617; *Eighth Annual Report of Florida State Geological Survey*, 1916, pp. 121–160, pls. 15–31; *The Journal of Geology*, loc. cit.

All of the bones are more or less broken and incomplete. The first bone found in place was the proximal part of the left ulna. An additional part of the shaft of this bone was subsequently found a few inches farther back in the bank. The second bone found in place was the proximal part of the shaft of the left femur. Two and a half months later, after the excavating had been carried back farther into the bank an additional part of the shaft of this femur was obtained, the two pieces of bone being separated in the bank by a distance of eight feet. This bone, the two pieces having been put together, is illustrated in figure 3 of plate 19 of the *Eighth Annual Report of the Florida Geological Survey.* The third bone found in place was the proximal part of the left radius. A photograph showing these three bones in place in the bank was reproduced in the *American Journal of Science,* July, 1916, and in the *Eighth Annual Report of the Florida Geological Survey,* plate 17, figure 1, and is included herewith (fig. 41). The two bones, left ulna and left radius, it may be noted, are separated by a space of five feet. The part of the radius preserved has a length of 145 mm., and hence the distance between the bones, as well as the thickness of the section, may be readily computed from the photograph. Vertically above the radius, as may be seen in the photograph, is twelve or fourteen inches of light colored, coarse, clean sand, with which is intimately mixed a quantity of broken marine shells, this part of the deposit representing material washed from the underlying stratum (no. 1 of the section). This is followed by about ten or twelve inches of vegetable material and sand, including as may be seen in the photograph pieces of drift-wood. Above this layer is seen a lens of coarse, clean sand including some pieces of broken marine shells, all of which has been thrown in by the stream. This sand lens, as seen in the photograph, has a thickness of about six inches. Above this sand lens to the soil line is found fourteen or fifteen inches of material consisting chiefly of muck, including some sand, the depth of this bone beneath the surface being about forty-two inches. In passing to the right the deposit of sand immediately above the radius "pinched out" so that the piece of femur which lies approximately four feet farther west is immediately beneath the muck, as is also the ulna.

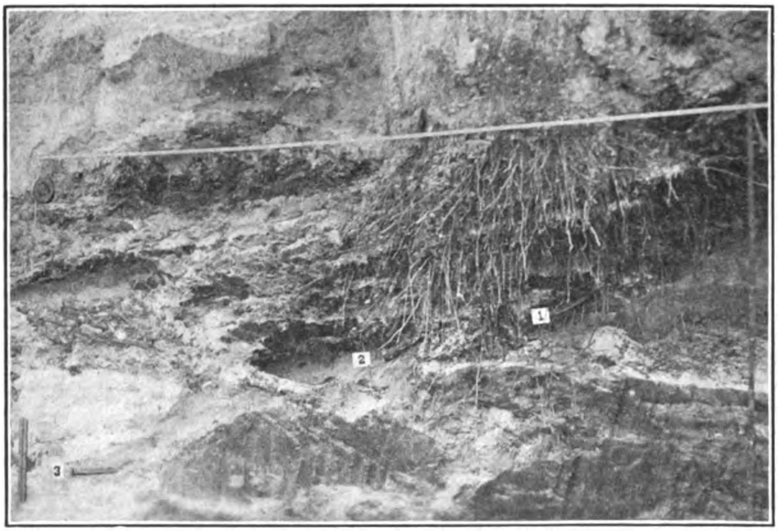

FIG. 41.—Photograph showing the ulna, femur and radius (nos. 1, 3 and 4 of fig. 39) in place in the bank. Scale of photograph, 1 inch equals about 2.2 feet. *Courtesy of American Journal of Science.*

In the writer's description of this locality the depth is given as four feet, which is not in excess of the thickness of the deposit overlying some of the bones. All of the bones definitely referred to this individual lie at the contact line of this deposit and the next older bed, and the varying depth of the bones beneath the surface is due to the fact that the top surface of the older deposit is irregular. The essential point involved, however, is not the depth at which the bones lie, but the fact that the deposits above the bones consist of alternating layers or strata which have not been disturbed. A sample was retained showing the sand in which the radius was imbedded, and also a sample of the sand including the broken shell vertically above the radius. This clean-washed, coarse sand together with the shell fragments, contrasting decidedly with the overlying accumulation of drift-wood and muck, affords positive evidence of stream-washed material, and conclusive proof that the deposit has not been dug into or otherwise disturbed.

The illustrations which accompany this paper include: a ground plan of the human bones found in place in the canal bank west of the lateral inlet (fig. 40); a photograph showing the ulna, femur and radius, all of which bones are broken and incomplete, in place in the bank (fig. 41); a closer view showing the succession of strata directly above the radius (fig. 43); a detailed view of the radius in place, including the sand and shell deposit immediately above it (fig. 42); and a photograph (slightly enlarged) of the sample of the sand and broken marine shells from immediately above the radius (fig. 44). These illustrations afford a record that is, it would seem, conclusive as to the conditions under which these bones were found. From the photographs it may be seen that flat objects, such as shells and shell fragments, lie prevailingly in a single plane of deposition, and that the deposits are cross-bedded, both of which features are characteristic of deposition by water. A study of the photographs and more especially of the section itself shows conclusively that these bones were washed by the waters of this stream to the place where found, and that they became entombed at the same time and in the same way as the sand, shell fragments, pieces of wood and other materials of this deposit. These bones are, therefore, unquestionably fossils of this formation and were not subsequently introduced into the deposit by human agency or in any other way.

After the photographs were taken (figs. 41–43) excavating at this locality was continued and human bones were collected over the area indicated in the accompanying sketch (fig. 40). The manner of occurrence of the human skull is instructive. Scarcely one half of the skull was obtained, and the pieces that were secured were distributed over an area of not less than seven by three feet. The broken skull fragments fit together securely. Most of the skull pieces were found in the sand containing the broken pieces of marine shells, and it is evident that they were washed to their present resting place in the same way and at the same time as the radius and the other bones The absence of bones and parts of bones is as instructive as the condition of the bones themselves. Of the jaw, for instance, there has been obtained only the right ascending ramus. This piece of bone is well preserved and the break shows a sharp fracture. There is no reason, therefore, to doubt but that the part of the jaw that is missing, if included within this formation at all, is also well mineralized. The same is true of the radius, of the left femur, and of the skull bones, as well as of the skeleton as a whole. From the time of the location of these bones in April, 1916, to the conference in October, 1916, the bank at this place was worked only by hand trowels and the material after being worked by the trowels was passed through screens, much of it being double screened through coarse and fine mesh. At no time were laborers allowed to work the bank with shovels or other implements. If the remainder of the jaw had been preserved within the area covered by this sketch, or in fact within a somewhat greater area, it would certainly have been recovered. The same is true of the missing and imperfect bones of the skeleton. These bones and parts of bones were either not washed into this formation, and for that reason failed of preservation, or if preserved in this deposit are lying somewhere outside of the area covered by this sketch.

It is evident that the bones of this skeleton had become thoroughly dried before they were moved and broken, this fact being indicated by the sharp fracture of the bones. Dr. Hrdlička has

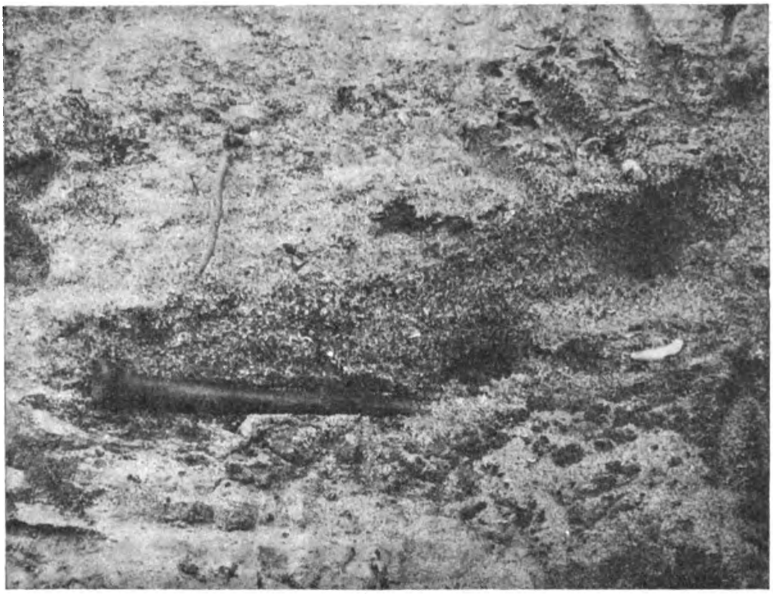

FIG. 42.—A view of the radius in place in the bank. Approximately one-third natural size.

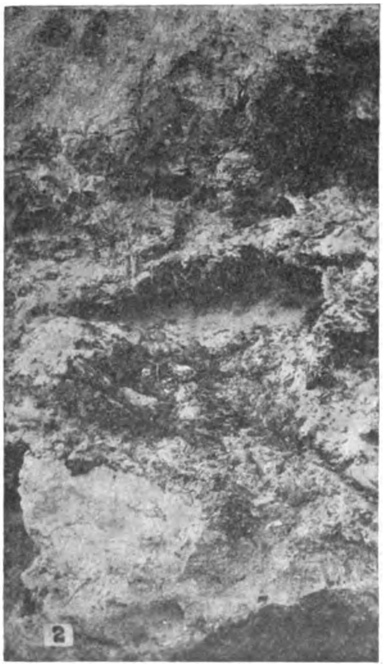

FIG. 43.—A view showing the succession of strata above the radius.

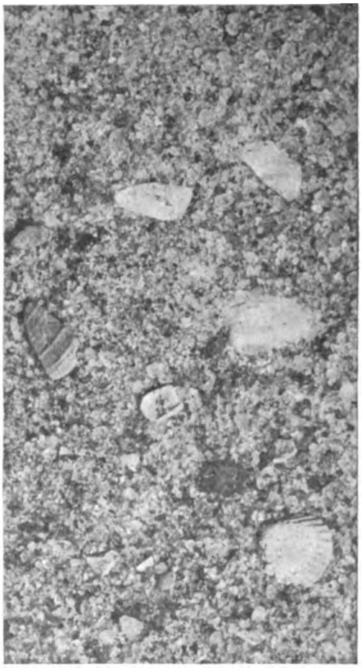

FIG. 44.—Photograph (taken in the laboratory) of a sample of the sand and shell from immediately above the radius. Slightly enlarged.

referred to the breaks in the bones as being "fresh," and suggests that the breaks may have occurred for the most part as the bones were uncovered and fell from the bank.[1] It is certain, however, that the breaks in the bones that were in place in the bank, including the left ulna, the left radius, the right femur, the left femur, the jaw and the bones of the skull, occurred at the time the bones were washed to the place where they were found. Some of the bones may have been carried much farther by the stream, while others possibly never found their way into this stream bed, thus accounting for the imperfection of the skeleton.

To assume that these bones represent a burial affords no adequate explanation of the separation of the radius and the ulna; of the displacement of the two parts of the right femur; nor of the broken and scattered condition of the skull as well as the scattering of the skeleton. On the other hand recognition of the fact that the bones were washed by the stream to their present resting place affords an explanation of every phenomenon that is presented, including: the broken condition of the bones; the separation of the radius and ulna a distance of five feet; the separation of the two pieces of the right femur a distance of eight feet; the position of the radius beneath fourteen inches of coarse sand and broken marine shells; the scattering of the parts of the skull; the presence of drift-wood in the deposit and the uninterrupted bedding above the bones, as well as the imperfect representation of the skeleton as a whole.

In all of its features this deposit maintains the characteristics of a stream fill, and we may plainly read the history of the accumulation of material at this immediate spot. The stream had cut into the marine shell marl (stratum no. 1 of the section, fig. 41), making a rather sharp trench in that formation. As the result of flood waters there was thrown into this trench an accumulation of coarse sand and broken marine shells which filled the bottom of the trench to a maximum depth of fourteen inches. Of the human bones the radius as seen in the photograph was left lying near the bottom of the trench, while a piece of the femur and the ulna as well as parts of the skull were thrown well upon the side. Quiet conditions followed, interrupted occasionally by mild flood waters. One of these floods threw in the lens of coarse sand, including broken marine shells, which is seen in the photographs (figs. 41 and 43) about twenty inches above the ulna. Under these alternating conditions of quiet waters and flood waters there was accumulated the successive layers of muck and sand, with occasional inclusions of drift-wood forming the stratified deposit which permanently sealed the bones and preserved them until the present time.

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[1] *Journal of Geology*, vol. XXV, 1917, p. 45.

The manner of occurrence of the pottery and bone implements must likewise be considered. Pottery is distributed throughout the deposit that has been designated as stratum no. 3, being more abundant, however, near the base. One hundred or more pieces of broken pottery have been taken from this formation. Bone implements are likewise general in their distribution although the greater number have been taken near the base of stratum no. 3. The large arrowhead illustrated in the *Eighth Annual Report of the Florida Geological Survey* (fig. 1, pl. 21) was found lying in a layer of light-colored, coarse sand at the base of stratum no. 3. The great abundance of pottery, bone implements and flints near the base of this deposit is accounted for in the writer's interpretation by the fact that the stream current was stronger when these first deposits were laid down than subsequently, and hence more material from the surrounding land surface was washed in than at a later time when the waters became more quiet. The muck which predominates in the upper part of the deposit belongs to the period of quiet and more or less ponded waters.

The muck of this section is followed stratigraphically by fresh-water marl, for while the marl is not everywhere present, the relative age is indicated by the fact that laterally the muck passes under and ultimately grades into the marl (*Journal of Geology, loc. cit.*, fig. 3, p. 10). The marl itself, although containing a few fresh-water shells and other fossils, represents chiefly calcareous material accumulated by chemical or bio-chemical processes. Its presence, therefore, is significant as to the probable age of the section. Hrdlička (*loc. cit.*, p. 49) refers to the fact that this fresh-water marl when first uncovered is often soft and hardens on exposure. This, however, is true of marls in general. The Ocala limestone of early Tertiary age is frequently soft when first uncovered and invariably hardens upon exposure. The same is true of many other limestones. He notes also the fact that shells piled up by the aborigines are sometimes found to have become cemented together. It is to be borne in mind, however, that the cementing of shells artificially piled up is a materially different matter to the accumulation of a marl by natural processes.

The human bones and artifacts that have been thus far referred to in this paper are, with the exceptions mentioned, those of stratum no. 3 of the section. The somewhat older bed, stratum no. 2, likewise contains human remains, flints and bone implements. The evidence that the human remains of this older deposit, as well as those of stratum no. 3, are contemporaneous with the formation in which they are found and do not represent recent burials has been presented in an earlier paper.[1]

ARE THE ASSOCIATED FOSSILS SECONDARY?

It is fully demonstrated the writer believes that the human remains and artifacts are not burials, and it thus remains to be considered whether or not the associated fossils can be secondary as suggested by Dr. Chamberlin. Dr. Chamberlin postulates that these fossils have been washed from the older Pleistocene deposits which lie immediately back of the beach through which the north and south forks of the stream cut, and refers to this formation as the "deposit which originally housed the old mammalian bones."

If the mammalian bones which are found in such abundance were washed from deposits further to the west, naturally we may expect to recover other and better fossils from the original or parent formation. Fortunately the opportunity for examining the formation in question for fossils is exceptionally good. The main canal after cutting across the beach ridge continues inland a distance of twelve miles. Moreover, the lateral which enters from the south continues for some miles in a general southwesterly direction. Paralleling the main canal and on the north side of it there is also a third canal which reaches inland about one eighth of a mile. The banks of these three canals which contain large masses of the particular deposit which Chamberlin regards as the source of the bones, have been carefully searched by the writer and others and no vertebrate fossils have been obtained. *It appears, therefore, that the formation from which Dr. Chamberlin would derive the bones is almost if not entirely non-fossiliferous.* In view of this fact is it to be believed that the abundance of bones found in the stream bed have washed from this formation?

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[1] *Journal of Geology*, vol. XXV, 1917, pp. 21–24.

In this connection the condition of the fossils themselves may be called into evidence. Both Drs. Hrdlička and Chamberlin have referred to the relative completeness of the human skeletons but there is obviously no point in this reference that will support the theories advanced by either of them. All of the human bones have been submitted by the writer to Dr. Hrdlička who states that he recognizes the presence of five individuals. One of these is represented by a single molar tooth; another by a single toe bone, while of another nothing appears to be known other than an incisor tooth. Obviously these three individuals could not have been represented by more fragmentary material. Of the two remaining individuals but an imperfect representation of the skeleton of each has been obtained, including twenty-six bones of the one and of the other scarcely so many.

Of the extinct wolf, *Canis ayersi*, thirty or more bones of a single individual have been found at one place, while near by was obtained the skull and femur probably of the same individual. The skeleton of this extinct animal is more fully represented, therefore, than is that of any one of the human skeletons. The extinct armadillo-like genus, *Chlamytherium*, is represented by a lower jaw, a bone from the skull and many dermal plates, all found at one place and probably all belonging to a single individual. The extinct stork, *Jabiru weillsi*, is represented by a humerus, part of a coracoid, part of two ulnas and two metacarpals, all found at one place and probably from one individual. Extinct turtles are represented by all or by parts of the carapace so fragile as not to withstand secondary deposition. The mastodon is represented by a part of the skull and tusk as well as by a lower jaw and by teeth. The elephant is represented by whole teeth and by parts of the skeleton. An extinct tapir is represented by a practically complete skull. There is in fact, as the writer has heretofore stated, no essential difference either in the completeness of the skeleton, or in the manner of preservation between the human bones and those of the associated animals.

The writer maintains that these fossils, including the skulls of the tapir, wolf and mastodon, and the carapaces of the turtles, washed to the place where they were found while they were still "green," and that after being "fossilized" and thus altered to their present fragile condition would not withstand being moved by currents of water. These fossils being shown to be contemporaneous with this formation, the argument for excluding the remainder of the fauna, including skeletons in varying degrees of perfection, largely loses its force.

The fact that the human bones found at Vero indicate, on the authority of Hrdlička, that the men of that time were structurally similar to the modern Indian has been used as an argument that these human remains can indicate no great antiquity. If, however, it is true that the Crô-Magnon race of Europe of some 25,000 years ago present but few structural differences from modern Europeans, why should we doubt that the much more isolated American race may have persisted through a longer period without sensible change in structure. It has also been objected that the flints, bone implements and pottery of this deposit are of types heretofore regarded as Recent, but it has not been shown at how early a date this culture type originated.

It should not be overlooked in this connection that the marine invertebrates of the underlying deposit (stratum no. 1 of this section) had at this early time become closely similar to the modern forms. Likewise, the land and fresh-water mollusks contemporaneous with the human remains are identical with the recent species. The notable changes are found among the vertebrates, particularly among the reptiles, birds and mammals, and include chiefly the disappearance of species, especially of the larger forms, in the extinction of which man himself may have taken an active part.

The evidence as it seems to the writer is conclusive that the human remains and vertebrate fossils found at Vero, are contemporaneous and that none of the objections that have been offered against this conclusion are valid. If it becomes necessary as a result of this discovery to recognize that the American race reached its present structural development and attained the cultural stage indicated by the artifacts of this deposit earlier than has been heretofore supposed, the conclusion is but in accord with other lines of geologic investigations which are constantly bringing to our notice larger time intervals and the earlier origin of types than had previously been assumed. To the writer the conclusion seems assured that the human remains and artifacts from the canal bank at Vero are to be referred to the Pleistocene period.

State Geological Survey,
Tallahassee, Florida

THE PROBLEM OF MAN'S ANTIQUITY AT VERO, FLORIDA

By GEORGE GRANT MacCURDY

DR. SELLARDS, whose discoveries at Vero, Florida,[1] have attracted so much interest, speaks with the authority of one who is thoroughly familiar with the site in question. To his foresight and the able assistance of Mr. H. Gunter, seconded by Messrs. Frank Ayers and Isaac M. Weills, their two local associates, we owe the recovery of new data bearing on the problem of man's antiquity in America. In view of the importance and complexity of the problem, Dr. Sellards very wisely sought the co-operation of anthropologists as well as additional geologists. The party invited to visit and report on the Vero site included Drs. Rollin T. Chamberlin, O. P. Hay, A. Hrdlička, T. Wayland Vaughan, and the present writer. While the various visits which took place the latter part of October overlapped, the results of the investigations of each were unknown to the others until after the

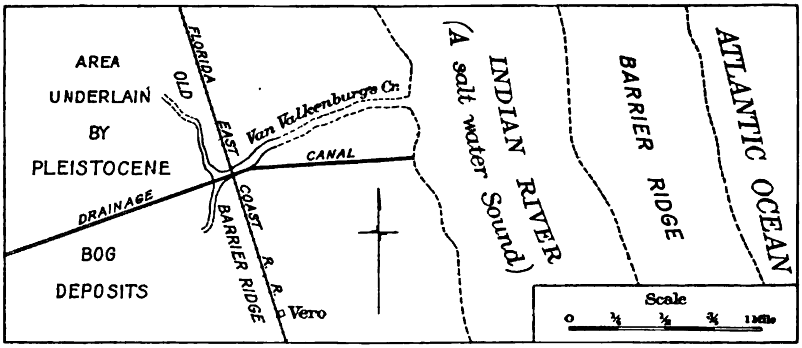

FIG. 45.—Rough sketch showing merely the general relations of the features discussed in the text. *Courtesy of University of Chicago Press.*

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[1] *American Journal of Science*, vol. XLII (July, 1916); *Eighth Annual Report Florida State Geological Survey*, 1916, pp. 121–160; *Science*, N.S., vol. XLIV (Oct. 27 1916).

publication of a symposium.[2] To this Dr. Sellards also contributed; his latest paper on the subject (p. 239 of this issue) calls attention to the diversity of views expressed by those who visited the locality. This diversity is due in a measure to the lack of coordination among the standards employed by the various observers; it proves alike the wisdom of cooperation in the effort to solve complex questions of this kind, and the unwisdom of basing conclusions on anything less than the sum total of the evidence. In the present case the evidence may not yet be all in.

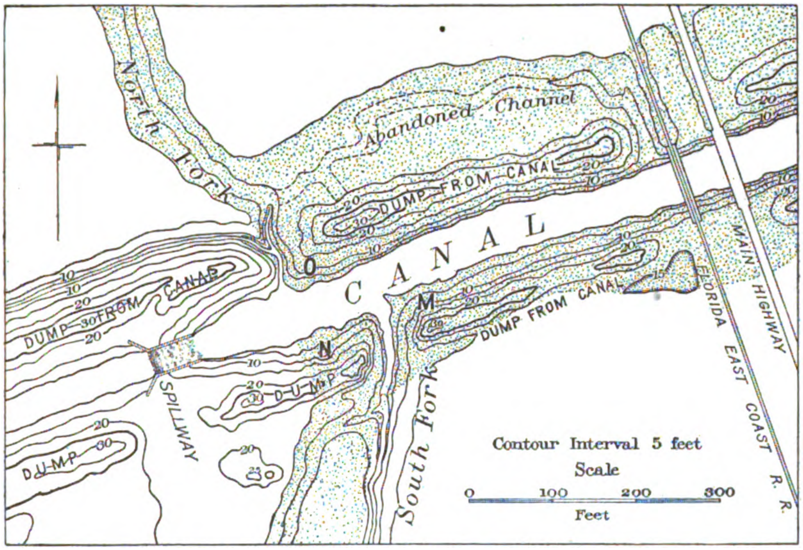

FIG. 46.—Detailed map of the locality where the human bones were found. The canal and the resulting dump piles have done much to change the original topography. The dotted area represents the flood plain of Van Valkenburg's Creek as it appears to have been just prior to the digging of the canal. The first human skeleton was found in formation no. 2 at point marked *M*, the second at point *N*, while human relics were found in formation no. 3 at point *O* as well as also near *N*. *Courtesy of University of Chicago Press.*

Briefly there are at Vero three deposits, lower, middle, and upper. The lower deposit, no. 1 of Sellards, is a marine shell marl. It underlies the stream deposits of Van Valkenburg's creek and the adjacent country. The marine invertebrates in this lower deposit are essentially the same as the modern forms. Above the shell marl according to Chamberlin the channel, or creek-bottom, section differs from the upland section.

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[2] *Journal of Geology*, vol. XXV, 1917, pp. 1–62.

The relation of the drainage canal to Van Valkenburg's creek and its lateral tributaries and the relation of both to the site of the discoveries are made sufficiently clear in figures 45 and 46, both borrowed from Chamberlin. It will be seen that the channel of the creek is relatively wide as indicated by the dotted area in figure 46; it is likewise shallow. Through it the waters followed a shifting and uncertain course until the canal was built. The old creek had cut into the shell marl or coquina, so has the canal. The latter has cut through the creek-bottom deposits for a distance of some 800 feet of the channel exposing the section already described by Sellards, and which Chamberlin calls the section of the creek-bottoms.

The middle deposit, no. 2 of Sellards, rests upon the shell marl. There seems to have been a slight interval of erosion between the formation of the middle deposit and that of the upper deposit (no. 3 of Sellards). This last has been interpreted by Chamberlin as a flood-plain deposit, its upper surface constituting the flood-plain of Van Valkenburg's creek prior to 1913, when the drainage canal was dug. The middle and upper are both creek deposits and are alike characterized by terrestrial animal and plant remains. Some of the animal remains in these deposits represent genera and species that are now extinct.

Of the human bones, parts of one and the same skeleton were found in the middle layer as well as along the line of contact between the middle and the upper layer (*N*, fig. 46). This is known as skeleton no. II. Human bones said to have been found in the second layer include parts of a skeleton that were removed by Messrs. Ayers and Weills before Dr. Sellards was notified of their discovery (*M*, fig. 46). According to Mr. Ayers these bones "were all close together, the whole not being over one and one-half feet in width. They were not scattered at all, nor piled up"; they are referred to as skeleton no. I. The only other human bones reported are an incisor tooth from the upper layer and found near skeleton no. II, a tooth of a child from the upper layer on the opposite side of the canal at O (fig. 46), and a toe bone of an adult also from the upper layer, likewise on the opposite side and some twenty feet farther down stream. None of the human bones differ in any way from Indian skeletal remains found in the sand mounds of Florida.

Most of the cultural remains were found on the south bank not far from skeleton no. II (at *N*); others were found on the opposite bank (at *O*), so that their distribution practically coincides with that of the human bones. In the present writer's opinion the markings on the tip of a proboscidian tusk and on the fragment of a bird bone, both from the middle deposit and previously figured by Dr. Sellards, are not the work of man and may be eliminated at once from the discussion. There remain a few flint spalls, an arrowhead, bone implements, a bone bead, and a considerable number of potsherds. Some of these specimens are here reproduced.

The flint spall seen in figure 47 is from the middle deposit, in the south bank and not more than three feet from certain bones of skeleton no. II. Another and smaller spall of identical material, which might well have been chipped from the same parent block, was found very near this one, but in the upper or muck deposit (fig. 48). It is significant that of these two chips of like material and so near each other in respect to horizontal displacement, one should have been from the upper and the other from the middle

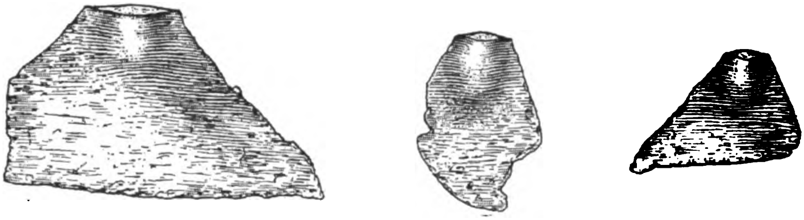

FIG. 47. FIG. 48. FIG. 49.

FIGS. 47–49.—(47) Flint spall from stratum no. 2, south bank, 460 feet west of the bridge and near human bones; (48) flint spall of identical material from stratum no. 3, south bank, 460 feet west of the bridge, from siftings; (49) flint spall from stratum no. 2, south bank, 460 feet west of the bridge, from siftings (½). Nos. 6964, 7072, and 7049. *Courtesy of University of Chicago Press.*

layer. The question arises whether both might not have been originally in the third horizon, one having later worked its way down into the second horizon by the aid of growing roots or burrowing animals. In places, roots do penetrate the middle deposit.

These spalls were never retouched or utilized. Each has what the French call a *plan de frappe* (plane of percussion) and a well-marked bulb of percussion. The inner or conchoidal surface is fresh and the edges are unworn. They were evidently chipped from the parent block not far from where they were found. At one time the presence of a bulb of percussion was looked upon as a sure sign of human agency. Certain rare examples from the base of the Eocene at Belle-Assise, Clermont (Oise), and from the Oligocene at Boncelles, Belgium, are proof that the bulb is not an infallible sign. By accidentally letting one flint fall upon another, the writer has on one occasion unintentionally caused the production of a bulb of percussion. It is however quite logical to assume that the vast majority of chips with bulbs, that occur in Pleistocene and later deposits have been produced intentionally, especially when associated with human skeletal remains or with undoubted artifacts. This is doubly true at Vero, because the source of the flint is the Ocala or the Tampa formation a hundred miles to the northwest of Vero. The cores from which the chips were struck could not well have been transported that distance over so flat a country except through human agency.

The small flint chip reproduced in figure 49, and thought by Sellards to be an implement, is likewise only a chip or spall with its plane of percussion and bulb of percussion. The multiple facets on its back or outer surface are due to the fact that it was an inner instead of a superficial chip. It also is from the south bank, 460 feet west of the bridge, hence from near skeleton no. II and the other two spalls here reproduced. While obtained from siftings, it is believed by Dr. Sellards to have come from the second stratum. A typical arrowhead of flint with barbs and stem, the latter however broken off, came from the contact line between strata nos. 2 and 3 in the south bank and near skeleton no. II.

A majority of the bone implements are from near the base of the third layer. Two found in siftings are thought to have come from the second layer; all however are of the same type (figs. 50–52). Figure 50 is a typical point from stratum no. 3, south bank, one of several from 450 to 470 feet west of the bridge. The fragment of a similar point, obtained in siftings from the second stratum, south bank, 462 feet west of the bridge, is shown in figure 51. Skeleton no. II was found 465 feet west of the bridge, in the midst of these various bone implements. Another and nearly complete point, obtained in siftings from stratum no. 2, south bank, 480 feet west of the bridge, differs from the other two only in size (fig. 52).

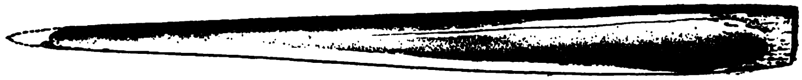

FIG. 50.

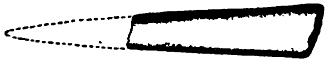

FIG. 51.

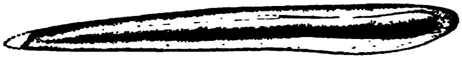

FIG. 52.

FIGS. 50–52.—(50) Bone point from stratum no. 3, south bank, 450–70 feet west of bridge; (51) fragment of bone point from siftings of stratum no. 2, south bank, 462 feet west of the bridge; (52) bone point from siftings of stratum no. 2 south bank, 480 feet west of the bridge (⅔). Nos. 6912, 6963, 6981. *Courtesy of University of Chicago Press.*

So far as the writer is aware no potsherds have as yet been reported from stratum no. 2, although they occur somewhat plentifully near the base of stratum no. 3. Of the dozen sherds kindly sent by Dr. Sellards to New Haven, every one is more or less waterworn. When subjected to stream action these sherds would show the effects of wear quicker than would the bones, flints, and bone implements. The pottery is of fairly uniform quality, the paste being neither crude nor fine. It is black to brown in color and the walls are of medium thickness. Judging from these twelve sherds, the ware was unpainted and undecorated. Of the three rim fragments, two are from bowls of medium size, the third somewhat thicker, is from a medium-sized bowl with slightly incurved rim. All these rims are plain but carefully finished. The smoke stains and accumulated soot indicate that these were culinary vessels. It should be recalled that the sherds, flints, and bone implements of stratum no. 3 are found in the north as well as the south bank of the canal at the junction of the two lateral valleys previously mentioned. None of these differ from similar antiquities found on the surface or in Florida mounds.

If we eliminate the specimens from siftings hitherto referred to the middle deposit, there is left in the way of artifacts nothing save the flint spall (see fig. 47). There has always been a question as to the status of skeleton no. I. In his paper of the present issue, Dr. Sellards ignores it.

Of skeleton no. II, most of the fragments recovered are either from the line of contact between the middle and the upper deposit or else had fallen from the bank, only a few pieces being actually found *in situ* in the middle layer. Absolute proof that this skeleton is as old as the middle deposit would therefore seem to be lacking, especially when we consider that it comes from the bed of a stream. Why should it be any older than the typical barbed flint arrowhead found near it and along the same contact line? The two isolated teeth and the one toe bone belonging to two other individuals were all from the upper bed.

In the present state of our knowledge therefore, all we can be sure of is that the oldest human skeletal and cultural remains at Vero are as old as the base of the upper deposit; or that they date back as far as the time interval separating the middle from the upper deposit.

It is now believed by Dr. Sellards that no considerable period of time intervened between the deposition of the second and third layers. Professor Edward W. Berry, of Johns Hopkins University, who has been studying the plant remains from Vero is of the same opinion. If this be true, then it is all the more probable that at least some of the remains of extinct animals in these deposits are derived from older beds. Dr. Chamberlin believes them to have come from the Pleistocene bog deposits directly to the west of the site (see fig. 45). From these bog deposits come the pebbles and cobbles of black sandstone found in the more recent middle deposit of the channel section. As yet however no animal remains seem to have been found *in situ* in these bog deposits, as pointed out by Dr. Sellards.

That Vero probably represents a concentration of fossil remains is indicated alike by the great variety and relative abundance of these, and by the topographic features of the place (see fig. 46). A concentration due solely to natural causes takes time, besides it opens the door not only to forms then or recently living but also to those of still older epochs. It does not follow therefore that man and the *Mylodon* were contemporaneous at Vero. The very fact that the medley of vertebrate fauna at Vero is so different from the present fauna of the region, while the human bones and artifacts are in every respect like those from pre-Columbian mounds, is a hint that should appeal to the cautious observer.

It is generally conceded that cultural remains are a much more sensitive chronometer than are animal remains. A lapse of time indicated by the faunal changes at Vero should be marked by even more profound changes in the character of the artifacts; but here we find nothing of the kind. The Crô-Magnon race has been cited as proof that the human bones from Vero are of Pleistocene age. But the race of Crô-Magnon is different in stature as well as bodily and facial proportions from the recent inhabitants of the Dordogne; while the Vero skeletons do not differ from those of the Florida Indians. Besides Crô-Magnon cultural remains are unlike those of the neolithic period.

After an examination of the plant remains from Vero, Professor Berry concludes that there is no appreciable difference between the ages of the middle and the upper layer, and that they represent a maximum age of three or four thousand years. He is sure that the two deposits are relatively very young, although some of the plants are indicative of conditions somewhat different from those prevailing at the present time in that part of Florida.

Thus far, nothing that would indicate post-Columbian contact with the native population has been reported from the Vero site; neither have copper or metal objects been found. From the mounds of Florida, Mr. Clarence B. Moore has taken various objects made of native copper and hence aboriginal. The absence from Vero of both these classes of specimens does not prove that the use of metals was unknown at that time; it should however be given due weight in a consideration of the age of the human bones and cultural remains from the site in question.

To summarize the evidences of man's antiquity at Vero, the human bones and cultural remains all have every appearance of belonging to one and the same age, as if they might even have come from the same village site or burying ground. They point to a period that might well have continued down to the close of the prehistoric period in Florida. In no respect whatsoever do they resemble the remains from the Pleistocene of any known part of the world.

The bone points are of a type that is widely distributed; they can be duplicated in collections from the southern mounds as well as the ruined pueblos of the Southwest. They represent the universal sewing implement of savage women and persist even to the present day. Their use is well known in basketry-making and other textile operations. The bone fish-hooks are exactly like those from scores of sites along the Atlantic coast as well as inland. In the Yale Museum are two such from an Indian grave at Beach Park, Clinton, Connecticut, which also contained arrowheads and a polished stone celt.

The same is true of the pottery, which even in its crudest form is never found in the horizon of the archaic argillite culture of the Delaware valley for instance. In a word there is nothing new or unexpected in the particular combination of cultural and human skeletal remains at Vero. Among the bone implements sent to New Haven for study was the sting of a sting-ray. This sting is a ready-made implement well known to the Indians; it is found in prehistoric sites along the Atlantic coast.[1] In the Academy of

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[1] On April 6 Dr. Sellards reported by letter the finding in the third layer, of a fragment from the head end of a slender bone pin with delicate crisscross incised

Natural Sciences, Philadelphia, there is a good example from a mound near Crystal river, Citrus county, Florida.

The antiquity of this combination of remains would seem to be fairly well represented in the figures ascribed by Professor Berry to the plant remains from both layers, namely a maximum of three or four thousand years.

It is only when one considers the formidable array of extinct mammals from the two layers in question that apparent lack of harmony arises; for even in the third layer we still have *Elephas columbi*, *Mastodon* (*Mammut*) *americanum*, *Chlamydotherium*, and a species of horse and of tapir. Here is the first element of uncertainty, for even paleontologists are not yet in a position to say just when these various forms became extinct in Florida. That the *Mastodon* lived on until recent times even in more northern climes, there can be little doubt. Of the extinct vertebrate fauna mentioned in Dr. Sellards' present paper, *Smilodon*, *Mylodon*, peccary, and camel do not occur in the third layer, and hence in the opinion of the author might properly be excluded from the discussion. If the extinct fauna in question was contemporary with the race that left the Vero type of culture, evidences thereof in other parts of the country, where both are known to have existed, would not still be lacking; unless the culture appeared earlier, or the fauna persisted longer, in Florida than elsewhere. Of these two alternatives, the persistence of the fauna is the more easily believable. At all events in the face of the irreconcilable differences between the combination of anthropological phenomena on the one hand and of paleontological on the other, and until there is forthcoming exact evidence as to the date of disappearance in Florida of the extinct vertebrates in this list, to say one is assured of the accuracy of the conclusion that the human remains and artifacts from Vero are of the Pleistocene period is to base one's conclusion on a forced correlation.

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decoration, also two perforated teeth of the shark. The pin is of the type found in Florida mounds. Neither is there reason to suppose that the two perforated teeth antedate the mound culture.

EDWARD B. TYLOR

By ROBERT H. LOWIE

EDWARD B. TYLOR, who died on January 2, 1917, at the age of eighty-four, had long been an historic personality. He loomed up as one of the very last figures rooted in the heroic age of nineteenth century science, as the peer and comrade in arms of Wallace, Huxley, and Spencer. The dean of ethnologists for two score years, he represented his science before students of other branches of knowledge and, thanks to the high literary quality of his style, before the cultured laity as well. He was read and cited by psychologist and historian, biologist and philosopher, by every one interested in the ways and thoughts of primitive man. And while the circle of his influence widened, he retained the profound and growing respect of his professional colleagues. Even with the irreverent group of American fieldworkers who turn up their noses at the classical school of ethnologists his prestige remains undiminished and their allegiance is of the kind he himself advocated,—no slavish acceptance of tenets but a following of methods "through better evidence to higher ends."

Edward Burnett Tylor was born at Camberwell on October 2, 1832, and educated at Grove House School, Tottenham. After a brief business career he traveled for several years and in 1856 visited Mexico in the company of Henry Christy, an anthropologist to whose personal stimulation he pays a generous tribute in the second edition of the *Researches*. The American trip led to Tylor's first publication, a book on *Anahuac; or Mexico and the Mexicans* (1861). Several years later appeared the *Researches into the Early History of Mankind and the Development of Civilization* (1865). This work laid the foundation of his professional fame, which reached its acme in 1871 with the publication of *Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Language, Art, and Custom*. In 1881 he wrote a most serviceable textbook on *Anthropology· an Introduction to the Study of Man and Civilization.*

Though not a university graduate, Tylor became connected with Oxford, both in the capacity of keeper of the University Museum and as a lecturer, being "reader in anthropology" from 1884–1895 and "professor" from 1895–1909, when he became an emeritus. Of the numerous honors conferred on him only two need be mentioned here. He was elected to a fellowship by the Royal Society in 1871 and knighted in 1912. A volume of *Anthropological Essays presented to Edward Burnett Tylor in honor of his Seventy-fifth Birthday* bore testimony to the regard of his fellow-workers. The bibliography concluding that volume indicates the extraordinary number of smaller and scattered contributions that fell from his pen in the course of years, and we learn with deep regret that a great work he had been preparing for many years was never published, which was also the fate of his ten Gifford lectures on Natural Religion, delivered at Aberdeen in 1889–1890.[1]

The most obvious feature that distinguishes Tylor's work from that of his English contemporaries and successors is the universality of his ethnological interests. Others, like Lang and Frazer, were predominantly occupied with sociological and religious problems; Tylor's vision embraced, to cite his own definition of culture,

> that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society.

He was equally attracted by the description of a Malagasy bellows and by an account of the South American couvade, by the process of stone-boiling and by solar mythology.

In Tylor's attitude towards the immense mass of concrete fact with which his versatility brought him into contact a distinctive psychological trait is manifest—his intuitive sense of fitness. We must recall the character of the data available when he commenced his life work—the hodge-podge of imperfect observation and provincial bias with which he was obliged to deal in order to get at the mere facts. To be sure, there was excellent material by men like Cranz, Sahagun, or Callaway. But even the most reputable of the older writers were prone to state as fact what was either crude misinformation at second-hand or crude misinterpretation due to the colored spectacles of European civilization. What shall we say when we find Burton declaring that the Arapaho possessed so scanty a vocabulary that they could hardly converse with one another in the dark when gestures were invisible, or Baker denying any form of religion to the aborigines of the Upper Nile region? In the evaluation of such utterances Tylor showed an almost unerring instinct, all the more commendable since many of the wild statements of this type would have fitted admirably into that general evolutionary scheme of the universe which he himself was helping to develop.

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[1] The biographical data are taken from Lang's sketch in the anniversary volume cited above and from Professor Haddon's obituary notice in *Nature* (Jan. 11, 1917), p. 373.

This critical judgment was apparent in the discussion of problems as well as in the weighing of travelers' accounts, but here the result was not so uniformly satisfactory. Indeed, the question obtrudes itself, whether Tylor's famous caution was not sometimes conformity to a scientific ethical ideal of fairness in discussion rather than a trait inherent in his mental make-up. He certainly carried the judicial weighing of pros and cons to an exceptional degree. On re-reading the *Researches into the Early History of Mankind*, I can understand Wallace's irritation at its indecisiveness and Lubbock's misunderstanding of the argument as to the single origin or independent development of the couvade. But whatever formal hedging there may be in the marshaling of arguments, the conclusion sometimes appears as a thunderbolt out of a blue sky, as when historical connection is used to interpret the existence in remote areas of the cure by extracting pathogenic agents from the patient's body.

This illustration, however, brings up a topic which shows Tylor to the greatest possible advantage in historical perspective. Though certainly a strong believer in the independent evolution of cultural phenomena in distinct areas of the globe, he was very much alive to the influence of diffusion. In the Introduction to the English translation of Ratzel's *History of Mankind* he contrasts "the small part of art and custom which any people may have invented or adapted for themselves" with "the large part which has been acquired by adopting from foreigners whatever was seen to suit their own circumstances." Indeed, in many concrete instances he goes much further than at all events modern American ethnologists are inclined to follow. The case of cure by suction has already been cited, while another chapter of the same book prefigures in principle the recent hypothesis of a cultural connection between aboriginal America and the Old World. Whatever we may think of particular interpretations offered by Tylor, the traditional American conception of him as merely an evolutionist of the classical school is ridiculously false. His suggestive and indeed conclusive discussion of the Malagasy iron technique alone suffices to show what a valuable tool he sometimes made of the principle of historical connection.

Nevertheless, it remains true that Tylor's name will always be most prominently connected with the doctrine of evolution. In this context it is very cheap to assume an unhistorically critical attitude. We must recollect that just as he had to sift the chaotic mass of ethnographic observations in order to extract the actual facts so in the interpretation of culture history he had to contend with a powerful, theologically inspired theory of degeneration against which the principle of progressive evolution had to be established and defended. To have accomplished this task so effectively is in itself no mean achievement to Tylor's credit. But Tylor further enriched the doctrine of cultural evolution by the development of a definite and elaborate scheme for the subject of religion. To enter into a discussion of this theory of animism is out of the question within the limits of this notice. Suffice it to say that as presented in *Primitive Culture* it remains, in spite of all criticism, the most impressive theory of primitive religion yet advanced.

To philosophical ethnology Tylor contributed the concept of survivals and the intimately associated method of "adhesions" outlined in his ever memorable paper "On a Method of Investigating the Development of Institutions; Applied to the Laws of Marriage and Descent," which was presented to the Anthropological Institute in November, 1888, and published in vol. XVIII (1889) of its *Journal*. It must be reckoned a distinct loss to science that the complete data on which this lecture was based were never published. The fundamental idea is the application of statistical methods to the data of ethnography. If two or more cultural traits are repeatedly found in association, are we dealing with a chance combination or is there an organic correlation? Tylor compares the number of times such combinations might be expected to occur on the doctrine of probabilities if each feature were independent of the others with the number of occurrences empirically found, and where the latter is clearly in excess he infers a causal connection. In this manner, *e. g.*, he establishes a functional relationship between the exogamous dual organization and the classificatory systems of kinship terminology, between the parent-in-law taboo and matrilocal residence, and between the couvade and a mixed maternal-paternal organization.

The very idea of introducing into a branch of knowledge that is so often the happy hunting-ground of the curiosity-seeking dilettante something of the rigor of the exact sciences is one of wellnigh unparalleled magnificence. Nothing that Tylor ever did serves so decisively to lift him above the throng of his fellow-workers. Without that paper he might have ranked as a sort of super-Lang or super-Frazer—more universal in his grasp than either, more serious and erudite than the one, far more trustworthy in his judgment than the other. But the paper on Method raises him at once into an entirely different category of intellectual being.

In the appraisal of this contribution several points should be considered separately. In the first place, quite apart from the main argument, Tylor here first conceptualized certain phenomena which have since loomed more or less prominently in ethnographic literature, *viz.*, teknonymy and cross-cousin marriage. Secondly, he was fully aware of the fact that it is one thing to establish the mere fact that two features are causally related and quite another to determine the reason for the association. The former is by far the more important methodologically and whatever criticism may be advanced against Tylor's specific conception of the nature of the correlation does not affect the core of the method. This likewise remains valid even if we reject the evolutionary interpretation which Tylor gave to certain of his observed correlations. Finding no instances of the couvade among matrilineal tribes, twenty cases among peoples with a mixed system, and eight in patrilineal communities, Tylor not only inferred that the institution had originated in the mixed system and dwindled away with paternal descent but also that this established the priority of matrilineal descent. Obviously, this conclusion does not follow from the empirical facts of correlation but already involves the acceptance of a unilinear scheme of evolution.

The essential objection to Tylor's paper, as pointed out in the oral discussion by Galton and Flower, rests on his neglect of diffusion. If the same combination recurs a hundred times among tribes that have had no historical connection, we have indeed established a rule of organic correlation; but if the combination has been disseminated from a single point of origin there is no means of proving that we are dealing with more than a mere chance association. We in America who accept diffusion to a considerable extent but at the same time admit independent development are confronted with the fact that exactly the same usages are found in remote regions of the globe between which any connection remains unproved. On the other hand, these similarities do seem to go hand in hand with certain other similarities, with which therefore they seem to be functionally related. This means that where one of the traits occurs, we can legitimately infer its one-time association with the correlated trait. We must insist against Tylor that the particular tribe in question may have borrowed the feature isolated from its old context; but to assert that such a correlation as that between the avunculate and a matrilineal organization is due to sheer chance is ridiculous, more so than the wildest Graebnerism, which at least does not blink at the observed fact of complete cultural identity. The best evidence for such an organic correlation seems to me to have been advanced in the field of kinship nomenclature, where Tylor himself established the relation of the classificatory system with exogamy. But the method is applicable to an indefinite number of similar problems, and ethnologists will do well to turn to Tylor's extraordinarily stimulating and fruitful mode of investigation.

Over and above his specific contributions, Tylor had a clear vision of the place of ethnology in modern civilization. The facts of primitive life were to him not mere specimens for a museum of psychological oddities nor was he altogether satisfied with using them as bricks for a theory of cultural development. Beyond its academic aspects he maintained that "such research has its practical side, as a source of power destined to influence the course of modern ideas and actions." The sight of mankind painfully groping through the ages from the crude fist hatchet to modern technology must inspire active endeavor to add to the heritage of the past. But ethnology also reveals in modern law, ethics, and theology innumerable survivals from primitive savagery, which it marks out for destruction, being in Tylor's own words "essentially a reformer's science."

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BOOK REVIEWS

METHODS AND PRINCIPLES

Kin, Kinship. (Hastings' Encyclopaedia of Religion and Ethics, vol. VII, 1914, pp. 700–707.) W. H. R. RIVERS.

Marriage, Introductory and Primitive. (Ibid., vol. VIII, 1915, pp. 423–432.) W. H. R. RIVERS.

Mother-Right. (Ibid., pp. 851–859.) W. H. R. RIVERS.

The first of these articles embodies the theoretical views familiar to readers of *Kinship and Social Organisation*, but also sketches the distribution of kinship systems in different parts of the globe and describes social functions connected with various relationships.

I think it is unfortunate that Dr. Rivers continues to use the word "system" not only as a conventional symbol for the entire nomenclature of a tribe but also connects with it the notion of some organic connection among the constituent elements. This certainly holds to some, but only to a very limited extent. There is an obvious correlation between my calling my father's brother "father" and his addressing me as "son"; but why classificatory systems should be associated with reciprocal terms or with the differentiation of brothers and sisters according to seniority remains entirely obscure. The truth of the matter simply is that there is *no* organic connection but a chance historical association. Both of the features occur with and without the feature of merging collateral and lineal kin, and each of the three must be dealt with independently of the others, as well as of a host of additional traits which appear in primitive terminologies. Any particular "system" is almost certainly always the resultant of a number of psychologically diverse and in the beginning historically distinct factors. Why not apply to kinship nomenclature the analytico-historical point of view of which Dr. Rivers is so prominent an advocate in other fields of investigation? The outline of the distribution of kinship systems would then be supplanted by one of kinship *categories* or *features*.

In the discussion of American systems Dr. Rivers lends support to the widespread notion that all the North American terminologies (except that of the Eskimo) are classificatory. In the absence of any synthetic attempt to cover the area neglected by Morgan, this is a venial error. However, it is important for American students to emphasize on every occasion that many tribes of the Far West are now definitely known to lack anything comparable to the Iroquois or Dakota terminology.

Very stimulating indeed is the treatment of the social duties and privileges bound up with certain relationships. Dr. Rivers rightly calls attention to our lamentable ignorance on such apparently obvious subjects as the social relations of parents and children in primitive society. It is curious to find the avoidance of brother and sister familiar to Americanists from the Arapaho and Crow instances turning up in Melanesia. Privileged familiarity is another American-Melanesian parallel. Though it may not have been recorded in print from other American tribes than the Hidatsa and Crow, it is known to occur elsewhere, *e. g.*, among the Dakota and Winnebago, and may turn out to be a widespread but neglected feature of aboriginal life. As found in some tribes, the relationship between maternal uncle and sister's son would seem to fall into the category of licensed familiarity. One omission that will probably strike most readers is the inadequate treatment of the social functions of relatives on the father's side which are prominent even in matrilineal societies. Thus, the paternal grandmother names the child among the Hopi, while the Crow and Hidatsa give presents to men and women of the father's clan. However, in the absence of satisfactory data from many regions of vital importance, we must be very grateful to the author for attempting a systematic survey, which cannot fail to be useful to later investigators.

The discussion of Marriage is remarkable for its sane conclusions on the subject of promiscuity. Says Dr. Rivers (§ 30):

> Lewis Morgan, who has been the chief advocate of an original state of promiscuity, based his opinion on evidence which we now know to be fallacious, and at present not only do we have no knowledge of any promiscuous people, but there is also no valid evidence that a condition of general promiscuity ever existed in the past.

For group-marriage the evidence is much better and Dr. Rivers believes that the classificatory system "is most naturally explained by its origin in communistic conditions." Nevertheless even this method of nomenclature has features intelligible from an earlier condition of monogamous or nearly monogamous marriages, which the author regards as probably the oldest form owing to the presumably small number of individuals in the primitive human groups. In short, he inclines to the view of the priority of monogamy and believes in the fairly wide distribution of sexual communism at a later stage, but does not hold that it formed a universal feature of ancient society.

By far the greater part of the paper is devoted to less speculative matters, consisting in a very useful enumeration of the principal forms of marriage and of their range of distribution. Dr. Rivers wisely points out that matrimonial regulations of actual kinship invariably coexist with those based on social groups of the clan or gentile type. Some widely quoted misstatements, such as that relating to the marriage of own brother and sister by the Vedda, are corrected. Naturally in the present state of the literature there are a number of omissions, to a few of which I should like to call attention. Cross-cousin marriage in Africa is not restricted to the Hottentot but occurs also among the Herero (Schinz, *Deutsch-Südwest-Afrika*, p. 177). Marriage with the mother's brother's widow is not merely inferential but was actually practised among the American tribes of the Northwest coast. In North America, at all events, it is not sometimes but very commonly that a man marries two or more sisters, even Morgan having noted at least forty instances. The Eskimo should be mentioned as being (in some localities) polyandrous. In America there is often not so much a purchase of the wife as an exchange of gifts by the two families. Finally, the custom of rendering service to the wife's family is probably rather more common than Dr. Rivers' statement indicates. It certainly existed among the Hidatsa (and presumably among neighboring tribes) and among the Tlingit (Swanton, *Twenty-sixth Annual Report Bureau of American Ethnology*, p. 424).

The article on Mother-Right disposes in exemplary manner of some widespread misconceptions that pervade popular literature, such as the notion that matrilineal descent is necessarily or often coupled with mother-rule. The author very clearly points out that in matrilineal communities women may even be debarred from holding property and merely "form the channel by which it is transmitted." He also shows that kinship with the father is universally recognized, that descent through the mother is compatible with patrilineal succession to office, that matrilineal descent and inheritance are consistent with the dominance of the father in household affairs.

There is a very instructive summary of the distribution of mother-right, which will remain the basis of later syntheses in this direction. The theoretical discussion, though naturally not definitive in the present stage of our knowledge, is markedly objective and gives great weight to American data. The association of patrilineal descent with local exogamy and also with totemism is emphasized, while on the other hand Dr.

Rivers suggests a connection between agriculture (rather hoe-culture) with mother-right. His correlation of the latter with the classificatory system is somewhat misleading, the essential correlation being really between the classificatory system and exogamous groups with maternal *or* paternal descent. Considering the theoretical importance of the point, I should have liked to see a fuller presentation of the evidence on behalf of the priority of maternal descent in certain parts of the world and also an expression of opinion on Tylor's famous line of argument. It is especially noteworthy and commendable, however, that Dr. Rivers does not believe in the general priority of mother-right, giving full weight both to the American objection on the ground of the high cultural status of matrilineal peoples and the diffusionist theory contravening a unilinear scheme of development. No fairer presentation of the case by a European scholar has come to my notice.

In conclusion I may express the hope that Dr. Rivers' three articles are but the foreshadowing of a much-needed systematic treatise on primitive social organization—a standard work to supersede Morgan's hopelessly antiquated *Ancient Society.*

ROBERT H. LOWIE

NORTH AMERICA

The Beothucks or Red Indians, the Aboriginal Inhabitants of Newfoundland. JAMES P. HOWLEY, F.G.S. University Press: Cambridge, 1915. Pp. 348, 37 pls.

Mr. Howley, the director of the Geological Survey of Newfoundland, has added another achievement to his record of research which earns for him a place in the foremost rank of the authorities on the history and physiography of the interesting but little known island of Newfoundland. The monograph on the Beothuk or Red Indians is distinctly a complete piece of work in so far as actual material bearing upon the history and relics of the unfortunate tribe is concerned. For a period of over forty years, the author has indefatigably pursued every clue that might throw light upon the history, ethnology, and finally the possible whereabouts of the tribe which disappeared from the stage of action before any systematic attempt had been made to study its culture. In the introduction to the work, and in several places through the volume, Mr. Howley undertakes to discuss the question of origin. He rather unnecessarily indulges in a series of speculations concerning the possibility that the Beothuk might be related to a pre-Indian race inhabiting North America, for instance the inhabitants of Atlantis (p. XVI). After discussing the several possibilities of Algonkian and Eskimo affinities. Mr. Howley subscribes conservatively to the theory of Sir William Dawson based on

> a tradition among the Micmac tribe of Nova Scotia that a previous people occupied that territory whom the Micmacs drove out, and who were, probably, allied to the Tinné or Chippewan stock. These, he thinks, may have passed over to Newfoundland, and become the progenitors of the Beothuks. This supposition appears to me to carry with it a considerable amount of probability. Here, isolated and undisturbed, for several centuries, untainted by intermixture with other tribes they could retain all their original traits of character, language, etc., which remained with them as distinctive features down to the last moments of their existence.

One is tempted to envy Mr. Howley, I should say, for this attitude of mind, even though cautiously expressed, which enables him, in the present state of our knowledge of northeastern America, to accept the probability of such a hypothesis. In the minds of ethnologists today the identity of the Beothuk has involved only two serious possibilities: one being that the tribe may have been a branch of the Algonkian stock, and the other that it formed an independent linguistic group.

Mr. Howley has chosen to present his material in chronological subdivisions. Beginning with the fifteenth century and running through the nineteenth, he quotes and discusses historical allusions to the tribe in a very thorough way. Quotations of the pertinent passages in the narratives of the Cabots (1497), Cortereal (1500), Verrazano (1523), Cartier (1534–5), Guy (1612), Whitbourne (1622) and a few other minor explorers, form the subject matter of the first twenty-five pages. These quotations are all very helpful and contribute to make the book what it purports to be, the most complete work of reference on the Beothuk.

In the next chapter, giving the occurrences concerned with the Beothuk in the eighteenth century, Mr. Howley discusses the tradition among the Micmac relating to the period of contact between them and the Beothuk (pp. 25–26). Another version of this tale is given on page 183. The significance of these, together with one that I also secured from the Micmac of Newfoundland in 1914, will be referred to for discussion later. A short narrative by Sir Joseph Banks (1766) containing some ethnological facts (pp. 27–28), and a full quotation of Cartwright's lengthy descriptive remarks (pp. 29–53) on the Red Indians (1768) and Mr. Bland's less important letters (pp. 56–59), all bring into the reader's hands the documents which he needs.

As regards the section of the work from page 62 on to page 130, one is constrained to say that while perhaps satisfactory to him who reads the entire volume through consecutively, it is rather disconcerting for the ethnologist whose interest is not at all sustained by the mass of references to such things as correspondence between officials on administrative details pertaining to the behavior of the colony towards the natives, the minutes of the Beothuk Institution, newspaper articles and the like. The section includes much repetitive matter in quoting opinions and suggestions written to those endeavoring to open up communication with the Beothuk. Action in this direction was stimulated by the reward which had been offered by the government to successful parties. The few facts of ethnology which are interspersed through this bulk of material (pp. 64–71 and 197–210) might have been advantageously excerpted and classified under topical headings with great economy to the reader. The incorporation of so much matter that is negligible, except perhaps for its sentimental historical value, really makes the presentation somewhat incoherent, because many of the letters and documents quoted here in full are hardly important enough to deserve mention. Aside from these, however, the author wisely reproduces certain well-known historical sources. Here follow, in complete form, Buchan's narrative (pp. 72–91), of his expedition to Red Indian lake to obtain intercourse with the Beothuk, and the interesting first-hand narrative which Mr. Howley secured from the lips of John Peyton himself (pp. 91–96), who captured a Beothuk woman named Mary March in 1819 and brought her to St. John's, the anonymous narrative (pp. 96–101) of "E. S." and another, concerning the capture, by Hon. Joseph Noad (pp. 101–2), together with Peyton's own report in 1819 (pp. 105–108). These introduce to the reader the pathetic history of the Beothuk woman who was taken captive at Red Indian lake and brought to St. John's, where she lived for some months in the care of a clergyman. Perhaps the most interesting section of the whole volume is that containing W. E. Cormack's narrative (pp. 130–168), in which Cormack describes how he accomplished the journey across the island with a Micmac guide (in 1822) in search of the Red Indians. His records are of the utmost scientific value, not only to the ethnologist but to the geographer and botanist as well. Next (pp. 170–175) follows an account of the capture of three females of the tribe, from one of whom, Nancy, or Shanawdithit, most of the linguistic material now extant was obtained. We learn that Mr. Howley had the opportunity of testing the glossary in part through acquaintance with a Mrs. Jure, of Exploits island, whom he "met in 1886 and who resided with the Peyton family at the same time as Nancy." Several short sketches recorded by individuals who were personally acquainted with the Beothuk woman are given (pp. 179–182). Following this is an account of the Beothuk Institution, founded in 1827, "for the purpose of opening communication with and promoting the civilization of the Red Indians of Newfoundland." From page 189 to 196 is another account by Cormack of his second futile expedition made in 1827 for the purpose of again attempting friendly relations with the Beothuk in the interior. Space is again given (pp. 197–210) to rather unimportant official correspondence. The reader in search of actual ethnological facts, however, will find a short section (pp. 210–214) given to this purpose. Another paper by Cormack, *History of the Red Indians of Newfoundland*, is quoted in full (pp. 222–229). The material here is largely historical and retrospective, repeating much that we have learned earlier in the volume. A few miscellaneous ethnological notes, attributed to Cormack, next follow (229–231). Mr. Howley, no doubt, out of associational interest gives biographical data relating to Mr. Cormack, who he believes must have left more manuscript on the Beothuk subject which is still waiting somewhere to be brought to light.

From page 238 to 251 is a series of accounts recorded from Shanawdithit, in 1829, containing the Indian version of Buchan's expedition in 1811, and outline sketches and diagrams of his movements. These are reproduced in plates. Sketches of ethnological objects, made by Nancy herself, are reproduced with descriptions and commentary by the author. Ten of these are figured, the originals being in the Museum at St. John's. They form, apparently, the most trustworthy source of documentary knowledge that we now have on the ethnology of the Beothuk.

Mr. Howley now devotes time, or more properly wastes time, in discussing "theories as to the origin of the Beothuks" (pp. 251–257). Cormack's supposition of Norse affinities, Latham's belief in Algonkian relationship, and Gatschet's conviction of independent origin, as well as a reference to possible Basque origin, are rementioned. A ludicrous essay quoted from a Mr. Sweetland's papers, in 1837, purporting to derive the Beothuk from a band of Tartars, occupies about three pages. We could admire Mr. Howley's historical method more if he had seen fit to denounce instead of perpetuating this matter in his book.

Three short essays follow (pp. 257–263) by the author on "Physical Features of the Beothuks," "Status of the Red Indian Women," and "The Custom of Using Red Ochre." The physiological data given in the first of these consist of remarks quoted from various authorities tending to show that the Beothuk were exceptionally tall, well formed, light in color, and different from both Indians and Eskimo. Another section (pp. 330–335) deals with physical material, of which two plates are given under the caption of "Finding of Beothuk Skeletons." Mr. Howley here gives a description of the finding of human remains in Notre Dame Bay by Mr. Coffin. Sepulchre was in a cave, the clothed body was enshrouded in a skin covering smeared with red ochre. It lay on its left side with "the legs bent up so that the knees formed a right angle to the body." Articles consisting of a human image of wood, two small bark canoes, miniature bows, arrows, paddles and a couple of small packets of red ochre tied up in birch bark, with smoked fish, accompanied one burial while near another, some distance away, were found some well-made stone spear and arrow-heads, a stone dish and some iron utensils. A birch bark roofing seems to have originally covered the remains. Several other burial finds are mentioned in a cave on Comfort island, Bay of Exploits, one near Rencontre, and another from near Bona Vista bay. In the description of the osteological material, however, no definite measurements are given, which is a lamentable omission because most of the specimens (including five or six skulls, I understand) are contained in the collections at the museum of which Mr. Howley has charge. It is sincerely to be hoped that someone will soon be given access to the skeletal material at St. John's, so that a properly detailed study can be made of it.

To return to the short section (pp. 262–3) dealing with the use of red ochre the author makes some general comparisons.

> Possibly the object of thus coloring the person and clothing red may have been the better to conceal their movements from the enemy or to render themselves less conspicuous when pursuing the chase, especially in the autumn, at which season the bushes and shrubs covering the barrens where caribou most resort, assume many tints of red and brown, corresponding closely with the red ochre of the Indians. Even the natural color of an Indian's complexion seems designed by nature to enable him more easily to approach game of any kind, etc. It appears to have been their universal practice to smear everything they possessed with this pigment.[1] Not only their clothing, implements, ornaments, canoes, bows and arrows, drinking cups, even their own bodies were so treated. Small packages of this material tied up in birch bark are found buried with their dead, and there is evidence even that long after the flesh had decomposed and fallen away, they must have visited the sepulchres and rubbed ochre over the skeletons of their departed kin. At least one such now in the local Museum was certainly so treated.

Mr. Howley, from page 265 to 288, presents considerable material

[1] Red ochre, *Odemet*, cp. cognate Wabanaki and Montagnais ***wlamán***.

entitled "Traditions Current among the Fisher Folk and Other Residents about the Aborigines or Red Indians." From personal talks with old people who knew either directly or by hearsay facts about the life of the Beothuk, the author quotes data presented in the shape of anecdotes. Several quotations of the same sort are made from published sources which would otherwise be inaccessible to the student who wishes to know everything that has appeared concerning the tribe. Most of the details given in these traditions refer to the inveterate hostility between the Indians and the whites, and, while the main interest in them seems to be in the narrative, we learn, nevertheless, of a number of interesting war customs. The depredations of the Red Indians form a frequent theme here and reprisals by the whites a still more frequent one. Mr. Howley did not omit questioning the Micmac Indians of the island about the Beothuk, although, I believe, had he made this a more definite object say thirty or forty years ago when he could have questioned older men, a great deal more could have been secured. There is such a miscellaneous mass of historical, personal and ethnological data in these anecdotes that a classification of the facts is much needed. Mr. Rand's version of the capture of a Beothuk woman by the Micmac, communicated to Mr. Howley in 1887, is given in full, and at the same time a similar narrative from the Malecite.

At the conclusion of the monograph (pp. 297–321) Mr. Howley reprints the three papers dealing with Beothuk linguistics, which contain all the vocabulary material extant, published by Dr. A. S. Gatschet in the *Papers of the American Philosophical Society*, 1885, 1886 and 1890. On page 322 Mr. Howley discusses the contents of Lloyd's papers on the Beothuk. Since Lloyd had compiled the historical references on the tribe, Mr. Howley, who does the same, has little to say except to comment on some negative traits—the absence of dogs and the non-use of narcotics. Lloyd's description of the implements which he found in a number of localities in Newfoundland are reprinted. Mr. Howley's closing contribution consists in giving us a bibliography taken from Dr. Gatschet's first paper on the Beothuk.

Eight halftone plates of stone implements and seven of bone implements and ornaments, together with the rest of the thirty-seven plates showing wooden and bark objects, form the body of illustrations.

In doing justice to Mr. Howley's volume so much space has been required in reviewing his actual content that only several of the more important matters of significance can be touched upon in discussion. The first of these concerns the cultural affinities of the tribe, and the second the present whereabouts of its descendants if there be any.

Among the prominent ethnological traits, which stand out to characterize the Beothuk, from the scanty knowledge that we possess, practically all may be said to correspond to traits encountered among either the Montagnais and Naskapi of Labrador or the Wabanaki tribes south of the St. Lawrence. The use of red ochre, for instance, among the Beothuk finds its parallel, to a certain extent, among the Micmac of Newfoundland today, also among the Montagnais, while the Penobscot showed a certain reverence for red paint and ancient burials in their region were made amid quantities of red ochre. The Beothuk conical, rectangular wigwam having a hoop around the inside near the top (pp. 29–30), is found with the same details in the whole region. The same is true of the rectangular winter log house (pp. 100, 211). Excavated wigwam pits supposed to be distinctive of the Beothuk (pp. 85, 245) occur in many sites in the Penobscot region. The deerskin hooded coats, with and without sleeves, boots made of caribou hocks (p. 212), snowshoes (p. 87) of the type described, antler and bone harpoons, lances, hide scrapers, arrows and needles obtained from archaeological sites, the birchbark vessels and receptacles (p. 249), the seal stomach oil receptacles (p. 246), bird-skulls preserved as fetishes (pp. 333, 340), bark burials (p. 214)—all of which are described in connection with the Beothuk, occur as characteristics in some or all of the surrounding Algonkian tribes. Even the deer fences (pp. 69–70, 152) mentioned by authors as something exclusively Beothuk, are also known to the Montagnais and Naskapi. If this category of positive traits could include the matters about which we know nothing, it would seem that Beothuk material culture would have to be classified as a part of the general northeastern Algonkian culture.

The inequalities of fate have caused a strange anomaly in the fact that whereas we have so little knowledge remaining of Beothuk material culture in general, we actually have more specimens of designs in decorative art than could possibly be collected among any of the surviving tribes of the northeast today. The simple and uniform art motives which crowd plates XXV–XXIX, and XXXVI, form a respectable body of material in this field. Comparison shows that the triangle series, diamonds, notchings and striated bars and other geometrical figures characteristic of Beothuk art appear also in the design category of the Wabanaki tribes.

It is most natural to suppose that during the last century many events took place in the interior of Newfoundland which were never heard of outside. The Indians are not accustomed to inform their white neighbors spontaneously as to what goes on in their own circles of life.

Accordingly it would be unreasonable in the extreme to lay stress on an assumed negative attitude in considering the question as to whether or not any of the Beothuk did assimilate with the Micmac-Montagnais population whose descendants still inhabit the island. Continuous and even intense hostility between tribes elsewhere in North America did not prevent intertribal marriages and culture borrowing, so why should the assumption of a similar procedure be dogmatically opposed in this case? From what we may judge from the consideration of native processes elsewhere, I think it more than likely that the two tribes in Newfoundland did intermingle more or less and that to a certain degree the ethnology of the present-day natives is apt to show some influence of the blending. Even in the scanty legendary material available from the Micmac and Malecite, Mr. Howley himself quotes one tale of intermarriage from the Micmac and several relations of capture (p. 285 and p. 286–7). How often such intercourse might have taken place, or how long it might have endured, we have no definite means of knowing except through vague expressions of opinion among the Indians, and further through three traditional accounts of a former period of friendship between the two tribes. Two of these references are given by Mr. Howley (p. 25–6 and p. 183).

Moreover, I feel partly assured that all of the Beothuk did not perish but that some of their descendants may be found among some of the bands of Montagnais or Naskapi in southern and eastern Labrador. This likewise is the trend of opinion among both the whites and Indians of Newfoundland today. Again, let us remember that in the beginning of the last century, writers such as Buchan (pp. 104, 226) and an anonymous author in the *London Times* (1820) (pp. 96 and 100), who knew the Beothuk, estimated them at several hundred. One can hardly agree with Mr. Howley's conviction that the Beothuk have been absolutely exterminated, in the face of so much evidence presented by authorities whom he quotes so frequently in his volume, of which the following is an example.

> Hon. Joseph Noad, Surveyor General of Newfoundland, in a lecture delivered by him in 1852, says, "That the Micmacs still believe in the existence of the Beothucks and say some 25 years ago (1827) the whole tribe passed over to Labrador, and that the place of their final embarkation as they allege, is yet discernible" (p. 257).

Howley adds in a note:

> On some of the old French charts of the northern extremity of Newfoundland, a track or path is shown extending along the low flat shore forming the south side

> of the Strait of Belle Isle, and facing the Labrador Coast, which is distinctly visible from here; being only about nine miles distant. This path is called "Chemin de Sauvage." There is also a place on the same shore called Savage Cove, which is probably the supposed place of their departure. This would seem to bear out the statement of the Micmacs. Again, in the English Coast Pilot for 1755, there is a place near Hawkes' Bay or Point Riche, called "Passage de Savages."

It is only fair to the future ethnologist to point out that while we have no definite reason to suppose that descendants of the Beothuk do actually exist anywhere now, there is, nevertheless, a strong force of opinion to render this an affirmative possibility. There is still a hope that a source of information on culture may be found. The ethnology of the interior of Labrador, especially that of the eastern section, is practically unknown and it would be very unsafe to say at this time what sub-types of northeastern culture and dialects might not be found there.

Whether or not the hope of learning more of the culture of the supposedly extinct tribe is ever realized there is a grave doubt if Mr. Howley's monograph will ever be superseded.

Frank G. Speck

ASIA

Grammaire de la langue khmère (cambodgien). Georges Maspero. Imprimerie nationale: Paris, 1915. Ouvrage publié sous le patronage de l'Ecole française d'Extrême-Orient, VIII, 490 pp.

This excellent grammar of Khmer, the fruit of twenty years of labor among the people, by an official in the Civil Service of Indo-China, marks an important event not only for the philologist, but also commands the serious attention of the ethnologist. The Khmer, whom we style Cambodjians (from Sanskrit Kamboja, the official designation of the country in native documents), number at present about 1,700,000 individuals distributed over Cambodja and the adjoining territories of Cochin-China, Siam, and Laos. The tribal name is of ancient date, being preserved in the transcription Ki-mao in the Chinese annals of the T'ang dynasty (618–906), and being recorded as Comar by the Arabic travelers from the ninth century onward. In former epochs the area covered by the Khmer was far more extended, and comprised the lower basins of the Mekong (from Luang-Prabang) and the Menam (from the wall of Kamphëng-Phet). Khmer domination in the latter region up to the eleventh or twelfth century is attested by ancient inscriptions found in Siam. The entire country now designated Cochin-

China was exclusively Cambodjian up to 1658 when it was overrun by the armies of the Annamese. The theory has been proposed that the Khmer should not be indigenous to their present habitats, but should be regarded as immigrants hailing from western regions, probably from India, while their present home, inclusive of Cochin-China, Annam, Champa, and the littoral of southern Indo-China, should have been the cradle of the ancestors of the Malayo-Polynesian stock, the Cham being its direct offspring, and the Malayans their emigrant descendants. Maspero points out that this thesis is based on purely linguistic data, and that no historical fact has as yet come to the fore to corroborate it. Profound anthropological studies will be required before the discussion of this problem may assume a definite shape. For the time being we can set the Khmer in relation only with a certain number of tribes surrounding them, on the basis of linguistic evidence. These are the Pear and Cong on the right bank of the Mekong; the Kuoy and Suoy or Sue on both sides of this river; the Stieng, Pnong, Krol, Tiom-Pueun, Rmang, Brao, Bahnar, Sedąng, Boloven, Kaseng, Alak, Ve, Kon-Tu, Ta Hoi, and Leung on the left bank of the Mekong. The ethnology of these primitive tribes, aside from some notes on the Pnong by Dr. Harmand, has not yet been studied. This group is enclosed in the north and west by peoples of the T'ai family (Siamese and Laos); in the south, north, and east by Annamese; and in the east, first, by peoples of Malayan speech, the Cham and some savage mountain tribes like the Raglai, Churu, Pih, Rade, and Jarai, second, by the important tribe of the Cho-Ma whose language seems to be intermediary between the Malayan and Khmer types of speech.

Further, Khmer and its dialectic variations form with Mon and a number of allied savage dialects a linguistic group which has received the name Mon-Khmer. The Mon on whom we bestow the designation Peguans, and whom the Burmese style Talaing were the inhabitants of the kingdom of Pegų that occupied the delta region of the Irawaddy, the north of the peninsula of Malakka, and probably part of the lower basin of the Menam, where it was contiguous to the Khmer kingdom. Conquered and subjected by the Burmese, the Mon since 1286 have ceased to form a distinct nation. The close relationship of their language to that of the Khmer is incontestably proved;[1] likewise to that of the Palaung, Wa, and Riang, primitive tribes in the basin of the Salwen.[2]

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[1] P. W. Schmidt, "Grundzüge einer Lautlehre der Mon-Khmer-Sprachen," *Denkschriften Wiener Akademie*, vol. LI (1905), pp. 1–233.

[2] Compare the appendix concerning these languages in P. W. Schmidt, "Grundzüge einer Lautlehre der Khasi-Sprache," *Abhandlungen der bayerischen Akademie*, vol. XXII (1904), pp. 778–805.

An attempt has been made also to link Annamese and Cham into the same group with Mon-Khmer, and philologists like Fr. Müller, E. Kuhn, and P. W. Schmidt have heralded the existence of a Mon-Khmer-Annam family. The students who attended the general linguistic courses at Columbia University in 1906–07 will recall the interest then evoked by a discussion of P. W. Schmidt's theories.[1] It has been apparent for some time that these, partially at least, are not built on a sound foundation, and to some extent are in need of a revision. Among the propositions submitted by the writer to the Research Committee of the American Association for the Advancement of Science at its last meeting was therefore included a recommendation that the program developed by P. W. Schmidt be made the guiding motive of a research worker in the field in question, chiefly as Schmidt's conclusions are partially based on limited and unsatisfactory material. In regard to Annamese, Henri Maspero,[2] a brother of Georges Maspero, has conclusively proved in a remarkable study that Annamese cannot be attached to the Mon-Khmer family. The alleged relationship was merely suggested by the coincidence of the numerals from six to nine.[3] Phonetically and grammatically, Annamese exhibits radical differences from Mon-Khmer. It does not share in the prefixes and infixes of this group, and moreover has developed a system of tones which in minute details agrees with that of the T'ai languages, whereas there is no trace of similar or any tones in Mon-Khmer. Whether Annamese for this reason should be classified in the T'ai family it is difficult to decide in our present state of knowledge. Modern Annamese presents an extremely complex structure, being the final product of a mixture of quite a number of different languages. Tied up between Khmer in the south, T'ai in the west, and Chinese in the north, it has been subject to an overwhelming influence from each of these groups. Primeval Annamese, with a foundation of its own, appears to have been evolved from the fusion of a Mon-Khmer and a T‘ai dialect, even with the addition perhaps of a form of speech as yet unknown, and subsequently with the layer of an enormous mass of Chinese words. The dominating influence to which Annamese owes its modern characteristics is to be attributed to a T‘ai language, and certain it is that Annamese is not Mon-Khmer.

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[1] "Die Mon-Khmer-Völker, ein Bindeglied zwischen Völkern Zentralasiens und Austronesiens," *Archiv für Anthropologie*, vol. v (1906), pp. 59–109.

[2] "Etudes sur la phonétique historique de la langue annamite," *Bulletin de l'Ecole française d'Extrême-Orient*, vol. XII, no. I.

[3] Hence E. Kuhn in his fundamental study, "Beiträge zur Sprachenkunde Hinterindiens" (*Sitzungsberichte der bayerischen Akademie*, 1889, p. 219), rightly inferred, "We are therefore justified in concluding that originally Annamese was foreign to the cycle of the other languages, and that its coincidences with these are traceable to subsequent influences,—a supposition which is well in keeping with historical conditions, as the Annamese appear to have started from the extreme northeast of the territory now inhabited by them."

In opposition to Schmidt, Cham and its congeners must likewise be detached from Mon-Khmer and assigned to the Malayan group; the Khmer elements in Cham are simply loans due to historical contact. I concur with G. Maspero in his scepticism as to the alleged relationships of Mon-Khmer on the one hand and Khasi, the Kolarian languages of Central India, Nicobar, Senoi and Semang on the other. The material at our disposal is still so incomplete and deficient that Schmidt's conclusions must still be regarded as somewhat premature, or at least be accepted with caution. I regret that G. Maspero has not pronounced an opinion on Schmidt's further theory of a connection of Mon-Khmer with Malayan (his so-called Austronesian group).

The earliest document of the Khmer language, known at present, is a twelve-line inscription yielding the date A.D. 629. Three hundred and fifty inscriptions and a large number of manuscripts written on palm leaves allow us to pursue the development of the language from the seventh century till the present. During this interval it has undergone no fundamental change, not even in its vocabulary; the noticeable modifications are all phonetic and of a normal character. In consequence of its monosyllabic formation, the language follows the tendency to reduce polysyllabic foreign words by contraction to a single syllable; for instance, *suor*, "heaven," from Sanskrit *svarga; pros*, "male," from Sanskrit *purusha; sas*, "religion," from Sanskrit *çāsana*.

The most interesting feature in the structure of Khmer is the formation of words by means of prefixes and infixes. In a former stage also suffixes seem to have been employed to a limited extent, but only few traces of these can be recognized (for instance, *leu*, "above," *leuṅ*, "to mount"); the language has lost consciousness of them. Some examples of prefix formation are:

leṅ, to play	— *p‘-leṅ*, orchestra.
em, sweet	— *pa-em*, sugared.
cap, to adhere	— *k‘-cap*, adhering.
san, peace	— *k‘-san*, peaceful.
cuor, furrow	— *p‘-cuor*, to till the ground.
ṅap, death	— *ro-ṅap*, to be extinguished.
sa, unconstant	— *saṅ-sa*, light-minded.

tiet, other	— *kan-tiet*, stranger.
seum, humid	— *an-seum*, dew.
bauk, hump of oxen	— *dam-bauk*, conical hill.
heṅ, dry	— *kra-heṅ*, cleft in dry earth.
mat, mouth	— *pra-mat*, to insult.
mul, together	— *sra-mul*, to join.
he, to follow in procession	— *daṅ-he*, to accompany the king.

Infixes are derived in the following manner:

krup, enough	— *k-om-rup*, to be sufficient.
kan, to hold	— *kʻ-m-an*, one who holds.
cam, to guard	— *c-m-an*, guardian.
deu, to march	— *tʻ-m-eu*, traveller.
ar, to saw	— *a-na-r*, a saw.
kal, to support	— *kʻ-n-al*, a support.
khe, month	— *k-aṅ-he*, season.
kem, varied	— *kʻ-l-em*, mixed.
tiṅ, balance	— *tʻ-l-iṅ*, to weigh — *t-aml-iṅ*, weight.
pĕk, division	— *p-ṅ-ĕk*, portion, *p-amp-ĕk*, to divide, *p-amn-ĕk*, piece, *p-rap-ĕk*, division.
kʻvak, blindness	— *k-aṅ-vak*, blind.
kʻlaṅ, strong	— *k-am-laṅ*, strength, force.
kʻcil, lazy	— *k-am-cil*, laziness.
krol, to roll from above downward	— *ke-d-rol*, cascade.
braṅ, black	— *be-d-raṅ*, soot.
pran, to bait	— *pe-d-ran*, bait.
kaṅ, circle	— *kʻ-v-aṅ*, curved.
dal, to husk rice	— *t-b-al*, mortar for husking rice.
cik, to dig	— *c-ran-ik*, hoe.

In respect to social customs and conventions, the excessive use of appellative nouns in the place of personal pronouns is of great interest. Their judicious employment is a veritable crux of the language and must be observed with rigorous attention. Maspero enumerates no less than forty-four forms of address varying according to sex, age, title, or function of the speaker or the person accosted. In addressing children different words are used for boys and girls, for babies, infants, small boys and girls, boys and girls in the stage of puberty, while the children have different pronominal expressions in speaking to father and mother. Functionaries, dignitaries, bonzes, kings, slaves, women have all distinct forms of address.

From a consideration of the numerals it may be inferred that Khmer has run through a threefold stage of development,—a quaternary, quinary, and vigesimal. Primarily the Khmer counted only up to four, for solely the numbers from one to four are common to all the members of the family. After their separation the Khmer proceeded to form a word for five, a monosyllable incapable of analysis. By means of the quinary system thus attained they succeeded in counting up to nine by the process of addition: five one, five two, five three, five four. In creating an unanalyzable root-word for ten they completed a decimal system which is logically carried out in the numbers from 11 to 19, framed after the scheme 1 + 10, 2 + 10, etc. Finally a monosyllabic expression for twenty, neither related to five nor to ten, was called into existence, and this represents their highest unit. Hence it should be supposed that all higher decimal units were based on twenty, but whether this was the case or not escapes our knowledge, as from the end of the thirteenth century the Khmer adopted the Siamese numerals from thirty upward. All other languages of the group (Mon, Stieng, Bahnar, Sedang, and Boloven) have common forms for the numerals from six to nine (but different from those of Khmer), and have evolved a decimal system. Like many other languages, Khmer, too, abounds in numeratives styled by Maspero *déterminatifs spécifiques*.

The verb is little developed, and does not offer much of general interest. A useful list of principal roots and their most common derivatives concludes the volume. The typographical work is perfect, and the new font of Khmer type designed, engraved, and cast by the Imprimerie Nationale, is very creditable. G. Maspero is also the author of a history of Champa (*Le royaume de Champa*, Leiden, 1914) and of an excellent history of the Khmer (*L'Empire khmèr, histoire et documents*, Phnom-Penh, 1904).

B. LAUFER

AFRICA

Anthropological Report on Sierra Leone. Part i. Law and Custom of the Timne and Other Tribes. Part ii. Timne-English Dictionary. Part iii. Timne Grammar and Stories. NORTHCOTE W. THOMAS. Harrison and Sons: London, 1916. III vols., 8°, pp. 196, viii, 139, xxx, 86.

Specimens of Languages from Sierra Leone. NORTHCOTE W. THOMAS. Harrison and Sons: London, 1916. Large 8°, pp. 62.

These reports by Northcote W. Thomas, government anthropologist, are in form and character like his earlier reports on Nigerian peoples. Sierra Leone, with an area of about thirty-one thousand square miles has a native population of about one and a quarter million. The tribes show little variation in the general mode of life; linguistically they fall into two great divisions, those speaking prefix languages and those speaking non-prefix languages. Of prefix languages are Timne, Limba, Bulem, Krim, and Kisi, the three last being closely related. Of non-prefix languages there are the quite sharply isolated Gola and the Mandingo group—Susu, Yalunka, Koranko, Kono, Vai, Loko and Mendi. The two largest tribal areas are those of the Timne and Mendi both of which come down to the coast. It is to the Timne that these reports chiefly apply. Mr. Thomas goes rather fully into demography, though it was difficult to secure reliable data. Polygyny here, as in Nigeria, seems to favor an excess of male births; twin births are uncommon but are not considered unlucky. The government, law, social organization, and religion of the Timne show little that is unusual or peculiar. Thomas's most interesting chapters are more on religion and magic. One deity, Kurumasaba, is fairly definite and there are vague notions of others sufficient for our author to suggest a heathen pantheon in former days. The general name *krifi* is used for a host of minor spirits, most of them without individual names, some good and others bad. *Satka* (offering) and *wanka* (protective rites) play an important part and there are many religious prohibitions, ordeals, divinations, etc.

The grammar here given is merely a summary of Schlenker, who worked among the Timne sixty years ago, but it goes beyond that missionary's published grammar as the Church Missionary Society put bulky manuscript material at our author's disposition. The vocabularies are in part recent, but also include matter taken from Schlenker, Koelle, and Knödler, the words not always being referred to the sources. The phonetic system used seems awkward and complicated but may be accurate. Thomas asserts that Koelle's system of transliteration is so complicated that he omits many of that old worker's diacritical marks. Thomas recorded some of his stories from the lips of his helpers, others are from phonographic records and of these he says:

> the text of transcription did not, however, on subsequent examination correspond to the phonographed text and the printed text is a compromise.

As Mr. Thomas has not aimed at "uniformity of spelling," has not had the opportunity to revise his data with a native helper, and has no knowledge of the languages recorded, we can only wonder to what degree this linguistic material is going to be of serious value for study. Mr. Thomas is a diligent and conscientious worker but his style is often confused. He has done much useful work and the British Colonial governments deserve credit for the series of publications which they have had him prepare.

FREDERICK STARR

Savage Man in Central Africa. ADOLPH LOUIS CUREAU. A. Fisher Unwin: London, 1915. 8°, pp. 351. Price 12 shillings 6 pence, net.

The author of *Savage Man in Central Africa* lived for more than twenty years in French Congo, in constant touch with native populations. He says:

> I examined them at every stage of their progress from the child of Nature pure and simple, to the citizen of their small kingdoms, and saw them sketching the outline of early society as though in a series of tableaux. I spoke their dialects, I lived their life, and was accordingly able to train myself to think their thoughts, as far as could be done by a mind so very different from their own.

Cureau does not aim to give precise and detailed descriptions of the different peoples he knew, but to paint a general picture. While analytic to a remarkable degree, he plans to make a synthesis of his whole series of observations. He realizes that students of individual tribes

> will find discrepancies of detail between their specialized observations, and my synthetic descriptions. I beg them earnestly not to forget that I am proceeding like a landscape painter. I deal with large masses only; I employ broad coloring; I get my perspective by that shutting of one eye which does away with the negligible incongruities and lets naught remain save a clearer, distinctive, more harmonious whole.

Cureau's observations are almost confined to Bantu populations but within the area of his knowledge they present notable differences.

> Can we ascribe to the same stock the brachycephalous Fans, and the dolichocephalous Bakongo, or the little Boteke of the upper Alima and the tall Bobangi? Consider the thickset brawny Bakamba and then the Basundi, with their delicate intelligent features, prominent foreheads, and limbs which are vigorous in spite of their slenderness. Look at the active Balali, tiny creatures, whose forms are almost feminine, and compare them with the tall Atyo, with their loose-jointed listless gait. How they all differ from the Bafuru, who are fine strapping men, with prominent chests, broad low foreheads and convex parietals, and still more from the tribes of the Central Ubangi, with their broad athletic shoulders and narrow skulls—the incarnation of brute force.

Within this Bantu mass, variable as it is, Cureau recognizes two fairly separate groups—the men of the woods and the men of the plains. They differ in their psychology as in their daily life. Our author's presentation of the psychology of the individual is masterly. It is an impressionist picture, but wonderfully true. He must have watched his black men with wonderful keenness. From the study of individual psychology, Cureau passes to society, studying the family and the village in a series of interesting chapters. After tracing the evolution of the village he considers it as an entity—physical, living, sentient, organized, moral. Only one who knows the African village can fully *appreciate* his descriptions, but any one can feel them. The village pulsates with life; it sees and hears, smells and touches.

> The village has a keen sense of hearing. News clears the spaces of the bush with extraordinary rapidity and is diffused like a subtle breeze. It flies, one knows not how, across rivers, forests and mountains, changing its shape at every stage until nothing is left of it but an almost unrecognizable outline. Three or four hundred miles are nothing to Iris, the harbinger of the Dark Continent, but though she starts out as a goddess she ends as a fish. Take no useless precautions to disguise your progress through a district, for it seems as though the ground has some marvellous resonance, which spreads an alarm ahead of you and when you get to some strange village you find that you are already expected. The rumble of the great alarm drums, cut in the trunks of trees, escorts your caravan from village to village and their muffled blows, whose conventional rhythm is punctuated like some Morse code, rise in many an echo, now deep and heavy from the neighboring copse, and again shrill and thin from far away on the horizon of the distant plains.

In a final division our author considers social organizations superior to the village.

Only a Frenchman could have written this book. In its symmetry and even development, its terse and brilliant style, its warmth and enthusiasm, its impressionism, it is purely French. It cannot be neglected by anyone who is interested in Africa and in the evolution of society. For after all, Cureau finds his savage man in Central Africa chiefly interesting as exemplifying human cultural development in general.

FREDERICK STARR

An Introduction to the Study of African Languages. CARL MEINHOF. E. P. Dutton and Company; New York, 1915. 16°, pp. vii, 169. Price, $1.75 net.

Carl Meinhof's *Introduction to the Study of African Languages* is an excellent manual and we are glad that Miss A. Werner has translated it into English. We are surprised that she did not find it convenient to preface her translation with some information regarding the work of its author. The book is composed of eight lectures, which are models of simple, direct, and clear statement. The propriety of studying primitive languages is first discussed. They lack the series of historical examples which, nowadays, is considered essential for the study of the great literary languages, but when considered in great groups and rigidly compared they are capable of showing, among and between themselves, the course of evolution quite as clearly. The languages of Africa fall into three or four fairly marked groups, which occur in zones or belts across the continent. In the south is the great Bantu family; north of it lies the Sudan family; still further north are the Hamitic languages; besides these great families there are the languages of the Bushmen, the Pygmies and the Hottentots. Meinhof sketches the history of the language study of Africa and indicates the directions in which the best promise for future investigation lies. He presents an interesting chapter on the phonology of African tongues and the relation of their sounds to those found elsewhere. Typical sentences are examined to show the structural characteristics of the different families. Meinhof has himself made important contributions to the knowledge of both Bantu and Hamitic languages. In the later lectures he shows the practical value of the study of African philology and its usefulness to other sciences. He finally attempts to show the relations of the three great families to each other and to the languages of other continents. The book contains an excellent map, drawn up by Dr. Bernhard Struck showing the distribution of African languages so far as at present known.

FREDERICK STARR

SOME NEW PUBLICATIONS

Bandelier, Adolf F. The Delight Makers. With an Introduction by Charles F. Lummis. Dodd, Mead and Co.: New York, 1916. xvii, 490 pp., 17 pls. Price, $2.00 net.

Benedict, Laura Watson. A Study of Bagobo Ceremonial Magic and Myth. (Annals of the New York Academy of Sciences, vol. xxv, pp. 1–308, pls. I–VIII.) New York, 1916.

Boas, Franz. Tsimshian Mythology (Thirty-first Annual Report of the Bureau of American Ethnology, 1909–1910, pp. 27–1037, 3 pls., 24 figs.) Washington, 1916.

Carnoy, Albert J. (See Keith, A. Berriedale *and*.)

Foote, Robert Bruce. The Foote Collection of Indian Prehistoric and Protohistoric Antiquities. Catalogue Raisonné. (Madras Government Museum.) Madras, 1914. ix, 262 pp.

Frachtenberg, Leo J. Siuslawan (Lower Umpqua); An Illustrative Sketch. (Extract from Handbook of American Indian Languages, bulletin 40, part 2, of Bureau of American Ethnology, pp. 431–629.) Washington, 1917.

Gifford, Edward Winslow. Tübatulabal and Kawaiisu Kinship Terms. (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 6, pp. 219–248.) Berkeley, 1917.

Hawkes, E. W. The Labrador Eskimo. (Canada Department of Mines, Geological Survey, Memoir 91, no. 14, Anthropological Series.) Ottawa, 1916. Pp. 235 (of which 35 are plates), 32 figs., 1 map.

Joyce, Thomas A. Central American and West Indian Archaeology. G. P. Putnam's Sons: New York, 1916. Pp. xvi, 263, colored frontispiece, 28 pls., 64 ills.

Keith, A. Berriedale and Carnoy, Albert J. The Mythology of All Races, vol. VI: Indian and Iranian. Marshall Jones Co.: Boston, 1917. ix, 404 pp., 44 pls., 5 figs.

Moorehead, Warren K. Stone Ornaments used by Indians in the United States and Canada. With Chapters by Arthur C. Parker, Esq. and Professor Edward H. Williams. The Andover Press: Andover, Mass., 1917. 448 pp., 265 figs.

Rea, Alexander. Catalogue of the Prehistoric Antiquities from Ādichanallūr and Perumbair. (Madras Government Museum.) Madras, 1915. xiii, 49 pp., 13 pls.

Rosen, Eric von. Träskfolket, Svenska Rhodesia-Kongo-Expeditionens Etnografiska Forskningsresultat. Albert Bonniers Förlag: Stockholm, 1916. 468 pp., 3 maps, 78 pls., 401 figs.

Swanton, John R. Significance of the Terms for Brother and Sister among Primitive Peoples. (Journal of the Washington Academy of Sciences, vol. VI, 1917, pp. 31–35.)

Waterman, T. T. Bandelier's Contribution to the Study of Ancient Mexican Social Organization. (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 7, pp. 249–282.) Berkeley, 1917.

DISCUSSION AND CORRESPONDENCE

CRITICISM OF "SOME VERENDRYE ENIGMAS"

IN the following pages I should like to offer a few criticisms on "Some Verendrye Enigmas," by Orin G. Libby, which recently appeared in the *Mississippi Valley Historical Review*.[1] There is a certain modern school of historians who appear to believe that the only satisfactory historical work to be done is the attempted demolishing of previously accepted facts; to the work of that school this Verendrye article appears to belong. Several years ago Dr. Libby's first article on the Verendrye problem appeared in the *Collections of State Historical Society of North Dakota*, vol. II, and his present paper is written with the assumption that its rather faulty theory was generally accepted. There are, however, many who did not accept it, and certainly to those who read the able review of the first article by Dr. R. B. Dixon in the *American Anthropologist*[2] the theory advanced would seem to be far from proven.

To those who can readily see that "Panani" is not Pawnee, that "des Missouris" is not the Missouri, it may appear clear that "Mantannes" are not Mandan and that "Panana" are Mandan—but it might also be contended under the same theory that "Assinipoels" could not be Assiniboine. Dr. Libby states that the name Mantanne refers to the Hidatsa tribe for the following reasons; that it is a Cree name, and that it is not the name by which the Mandan people call themselves; and he concludes that it came to be transferred to the Mandan at some date after 1743, when the younger Verendrye last saw the tribe, in some unknown and mysterious way. Let us examine the history of this name. We find that a Dakota name for the Mandan people, as given in the *Handbook of American Indians*,[3] used from time immemorial, is Mawatana, another form Mawatadani; the Assiniboine form of this is not at hand and may have approached even closer to the one used by Verendrye. We find the form Mandans applied to the Mandan tribe in 1772 by McKenzie, the form Mandons appears on a map in 1794, Trudeau speaks of Mandans in 1795, Le Raye refers to the tribe as Mandans in 1801, Lewis and Clark and Henry call them Mandans in

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[1] Vol. III, no. 2 (Sept., 1916), p. 143.

[2] *American Anthropologist* (N. S.), vol. II, no. 3.

[3] *Bulletin* 30, *Bureau of American Ethnology*.

1803–04, and Perrin du Lac uses the form Mandanes in 1805. We find the Verendryes using Mantannes from 1738 to 1743; this leaves a lapse of but twenty-nine years for the mysterious transference of names to take place and for the name to become firmly fixed on its new host.

On the other hand we find that neither did the Hidatsa ever call themselves Mantannes, their own name for themselves is Hidatsa, and they were also called Minnetaree and Gros Ventre—these names were fixed at least as early as 1790 when Charbonneau first arrived among them; nor is there any historical nor traditional evidence to show that they were ever called Mantannes.

Dr. Libby also fails entirely to discuss the names Panana and Panani. The Mandan in 1738 told Verendrye that the next tribe below them was the Panana with the Panani next beyond them, that these two tribes had been one until quite recently, when they had quarreled and separated, the Panana going on northward. We find in the *Handbook of American Indians* that the words Panana and Panani are both Dakota names in good usage for the Pawnee tribe; the old timers along the Missouri in North Dakota very frequently mention the Arikara as Panana even now. Moreover the story of the quarrel and separation fit both chronologically and circumstantially with the quarrel between the Pawnee and the Arikara. However Dr. Libby sees no good reason why the Panana should not be identified as Mandan. As he pictures it there must have been a general shift of names along the Missouri in the middle eighteenth century something like a game of progressive whist. We know that there were French traders among the Pawnee in 1700 who knew the tribe as Panani or Pani so that that tribe must have failed to progress. If there were proof that the Panana and Mandan were one, and that they were part of the Pawnee nation shortly before 1738 we should certainly be indebted to Dr. Libby for a wonderful discovery which would revolutionize all our ideas of linguistic stocks.

Dr. Libby lays much stress on the value of traditional evidence from the Indians in supporting his theory; very little of such evidence is cited, however. In fact in our own efforts to arrive at some sort of solution of "some Verendrye enigmas," we have done a considerable amount of questioning and inquiring from different Indians, all of which has elicited no information at all with respect to Verendrye either from the Mandan or the Hidatsa.

Another rather obscure feature of Dr. Libby's paper is the matter of the movements of the Hidatsa tribe. He states in one place that the Hidatsa were known to have lived in Canada before coming to the

Missouri. This statement would bear proof, for acquaintance with their migration myths and traditions, as told by a number of individuals, gives no clear story beyond the residence in the vicinity of Devils lake. The question of time is also somewhat involved; it is first stated that the Hidatsa did not leave the Devils lake region until the last half of the eighteenth century, Dr. Libby then proceeds to discover them at a distance of over one hundred and fifty miles west of Devils lake in the first half of that century, near the mouth of Little Knife creek on the Missouri. It is very probable that both of these findings are incorrect. Hidatsa tradition and Mandan as well give rather elaborate accounts of the coming of the Hidatsa to the Mandan on the Missouri at the mouth of Heart river, where both people say that the Hidatsa first learned to cultivate the soil, and raise corn, beans and squashes. The crossing place of the Hidatsa, mysteriously mentioned, but not divulged by Dr. Libby, has been placed by a number of Indians questioned, as very close to the spot where the Northern Pacific bridge now spans the Missouri; there is also at least one of the Heart river sites which is identified by the Indians as Hidatsa.

The Hidatsa had lived on Heart river for some time before the quarrel took place which resulted in the separation from them of the Crows. Another considerable period elapsed before the Heart river region became so crowded that, at an intertribal council, it was decided that the Hidatsa should move northward to Knife river. It should be borne in mind that these events all occurred before the great smallpox plague of 1782. That plague broke the strength and power of the Mandan and by 1795 they were located in the Knife river region near the Hidatsa. It certainly seems highly improbable that all the events from the arrival and settlement of the Hidatsa on Heart river to the final settlement on Knife river, accounted by the Indians as covering a period of many years, could have taken place in the comparatively short period of thirty-nine years between the time that the younger Verendrye left in 1743 and the year of the smallpox—1782. It is very much more likely that the Hidatsa were already established close to the Mandan in 1738, and that Verendrye, without an interpreter, or any means of communication but signs, failed to apprise himself of the fact. It is significant that the words Mantanne, Panana, and Panani are all of Dakota origin and were evidently furnished by the Assiniboine although the information about them may have come from the Mandan. The two tribes may well have been living together, but with the Hidatsa villages west of the Missouri, as they were, Verendrye's attention would not have been attracted to them.

That the Hidatsa built villages north of Knife river at various places we learn from traditions, notably one near Expansion, N. D., and one near the mouth of the Little Missouri river on a high hill, called Night Walk's Village; we are also told that these were inhabited only for short periods, and that they were built after the Hidatsa had left Heart river and settled on Knife river. It is more than probable that the village on Little Knife creek belongs to this group; a significant item is the statement that the old garden can still be pointed out at this site, good evidence that the Hidatsa had already learned agriculture from the Mandan. If, as Matthews states, a statement quoted by Dr. Libby, the Hidatsa lived at Heart river with the Mandan in 1764, these sites must be fairly recent and built long after the time of Verendrye's visit.

Let us turn now to the location of the Mandan villages visited by the Verendryes. The only evidence, which, if correct, is favorable to the Little Knife site as that visited by Verendrye is the latitude given in his account. However, practically unsupported by other evidence, and contradicted by the distances given, as shown by Dr. Dixon in his review of Dr. Libby's first Verendrye article, it can not be given too much weight. It is the only latitude given in either Verendrye account and the breaking of their instrument is mentioned by the younger Verendrye. It is more than possible that the figures may have been carelessly transcribed in the document preserved, or that their instrument may have been deranged so that incorrect results were obtained, as happened in the case of other early explorers. Dr. Libby lays stress on the location at the mouth of Little Knife creek as being a point from which the Missouri might be mistakenly supposed to flow southwestward. There are however four other points between there and Heart river, at three of which are old village sites, where casual observation might give rise to the same mistake; these are—near the mouth of Wolf creek, just below Washburn, N. D., and just below Glencoe, N. D.

The last location seems the most probable one for the Verendrye visit, as the site here belongs to the Heart river group, and there is a definite report of an inland site on a hill some fifteen to eighteen miles back from the river site, and fitting fairly well with the location and description given by Verendrye of the Butte village. This inland site is mentioned in print by Mr. Jos. Taylor who visited it in the early seventies, and has been orally described for the North Dakota State Historical Society by Mr. Dan Williams who was with Mr. Taylor at the time the site was discovered.

Another feature relied upon as good evidence with not very great reason is the question of the internal village arrangement. That there is a difference between the three different tribes in village arrangement, that is the Mandan, Hidatsa and Arikara tribes, is certain. The Hidatsa village so far as any historical accounts, or the survey of any known Hidatsa sites show, was arranged without order and without anything at all which Verendrye might have called streets or squares. The Mandan site on the other hand, did have at least one large open space, and had a sacred lodge or "holy tepee" slightly different in construction from the other lodges, on one side of this open space, but placed between two other lodges of the usual sort in the circle of lodges about this open area. Dr. Libby declares that Verendrye could not have failed to see and report upon this sacred lodge. However, we know only too well that he failed to report on very many important facts; and he is not alone in the failure in this case for we find that Le Raye, Lewis and Clark, Henry and several others of the early visitors to the Mandan fail to give any account of this feature.

Turning now to the journey of the younger Verendrye, we find a notice of the various colored earths which he remarked as he traveled from the Mandan villages. He might have seen much of this colored earth scenery in the more northerly route suggested by Dr. Libby, but he would have seen it in even more striking display on a course in the direction named, from Heart river, which would take him by way of White Chalk buttes, Cave hills, Slim buttes, and Long and Short Pine hills.

There is of course much discussion as to what mountains were visited on this journey and, certainly, from the involved way in which the dates and distances are given, it will always be a debatable question. However it takes a considerable stretch of the figures to bring the party to the Big Horns, so that the Black hills would appear the most probable range.

The identification of the tribes visited by the younger Verendrye is another puzzling problem. The fact that the Gens des Chevaux were expected without fail at the Mandan villages would naturally suggest that this people was the Crow nation who always from the time of leaving the Missouri, made regular visits to the Mandan and Hidatsa villages. The term Pioya may possibly refer to the Kiowa; of the Little Foxes, Gens de L'Arc, and Beaux Hommes, identification is almost impossible without more data than is now accessible. We are told also of the people "de la Belle Riviere," which at least gives some geographical evidence, for the north branch of the Cheyenne river was even then known as Belle Fourche or Belle Riviere.

There seem, in spite of the counter arguments, good reasons for identifying the Cherry People as the Arikara. Verendrye meets them returning from winter quarters, in very nearly the same locality where Le Raye met the Arikara in 1801; both travelers accompanying them back to their villages on the Missouri. We are told[1] that the French traders found the Arikara living below the Cheyenne river in 1770, and we have definite mention in Lewis and Clark that they had villages near the mouth of Bad river during the eighteenth century. It is highly probable then, that at the time of the younger Verendrye's sojourn with them the Arikara were dwelling in villages about the mouth of Bad river.

Dr. Libby presents as evidence against the conclusion that the Cherry People were the Arikara the statement that the Arikara never wintered far from the summer villages. We have evidence to the contrary in the story of Le Raye, above referred to. We know also that the related Pawnee usually went on long winter hunts, and it is quite probable that, at that time when the constant danger from the Dakota was still to come, the Arikara did the same. Even as late as Boller's day we find the Arikara traveling some thirty miles from their Ft. Clark village to build their winter quarters and at the same time the Mandan and Hidatsa went even farther.

We come now to the matter of the lead plate, deposited by the younger Verendrye as described in his report, and recently discovered on a hill near the mouth of Bad river in South Dakota in the vicinity of the village of Ft. Pierre. Dr. Libby tries to explain the fact that the plate was found here as a purely fortuitous circumstance. On the contrary, from what has already been said, this location would be the natural place to find the plate, and exactly the locality where it was originally buried. With regard to the pyramid of stones which the journal tells of building at about the same time; although Dr. Libby goes on the assumption that it was built above the buried plate, yet the account does not say so; and the fact that the place of burial was desired to be kept a secret would certainly go to show that the pile of stones was erected upon another spot. As to the permanency of such a thing as a pile of prairie stones, Dr. Libby in his remarks about its being torn down to see what it concealed, gives sufficient evidence of its instability.

This brings us to the matter of the journey from the country of the Cherry People to the villages of the Mandan. Dr. Libby considers it surprising that, if the Cherry People were on the Missouri, they did not tell Verendrye of the Panani and Panana above them. Considering that they were the Panana themselves the surprise at this would be rather diminished. If Verendrye traveled from the country of the upper Platte, as Dr. Libby contends, he would have traveled rather east and northeast than north-northeast and northwest. The above course, however, would just fit the route from the Arikara villages at the mouth of Bad river to the mouth of Heart river.

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[1] "Handbook of American Indians," *Bulletin 30, Bureau of American Ethnology*, article on the Arikara.

It is contended also that the party could not have followed the course of the Missouri because they did not meet the Panana whom we have shown that they had already left behind. Dr. Libby overlooks the fact, however, that they did meet one tribe, and this meeting is very valuable evidence. The account tells rather circumstantially of meeting with the Cheyenne, to whom, fortunately, they give the French name by which they were generally known. The Cheyenne were camped temporarily, and pointed out the spot where they intended to build their permanent village. We have corroborative historical and traditional evidence showing that this was just the period when the Cheyenne crossed the Missouri and built their last earth lodge village between the villages of the Arikara and the Mandan.

There is no question of the value of traditional evidence secured from the Indians, and much of such evidence has been used in the preceding pages. It becomes infinitely more valuable, however, when coupled with the careful reading of all written accounts.

GEORGE F. WILL

BISMARCK, N. D.

MOCCASINS

DR. HATT became favorably known to many of us when during 1915 he visited the museums of this country for the study of North American and North Asiatic costume, for which task he was well prepared by his book *Arktiske Skinddragter* and an article "Mokkasiner" published in the *Geografisk Tidskrift*. We are grateful to the author for his courtesy in giving us the benefit of his present researches in the English language.[1] Hatt's method is excellent, and his enthusiasm for his subject is supported by a wealth of solid information, a thoroughness and keenness of analysis that is deserving of high praise. His study is divided into two parts,—a descriptive one in which all available types of moccasins and related foot-gear are described, illustrated, and classified, and a theoretical discussion of the origin of the moccasin group. As the material and the theory based upon it cover both North America and northern Asia, it becomes incumbent both upon Americanists and students of Asiatic ethnography to define their attitude toward Hatt's results. In order to preserve full impartiality, I shall not await the Americanistic verdict, but will briefly set forth my opinion solely from the Asiatic point of view. It may be taken for granted that Hatt's treatise is in the hands of all readers of the *Anthropologist*, and that a detailed analysis of its contents is therefore superfluous.[1] Dr. Hatt says;

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[1] "Moccasins and Their Relation to Arctic Footwear," *Memoirs of the American Anthropological Association*, vol. III, no. 3 (1916), pp. 149–250.

> The material at hand enables us to say that the moccasin group has *not* furnished the prototypes for all the forms of footwear, which we find in Central Asia and Asia Minor, it is even quite probable that *none* of them were derived from a moccasin type (p. 236).

While Dr. Hatt is very keen at recognizing moccasin types, it is striking that he fails to grasp the origin and culture-historical position of other types of footgear. Not only has that of Central Asia positively nothing to do with the moccasin, but also it has had a quite different development. What this history was we recognize clearly from ancient Chinese records. King Wu-ling (325–299 B.C.) of Chao introduced into China the tactics of mounted infantry, as it was then practised by the nomadic tribes (Turkish and Tungusian) of inner Asia. This innovation was a military necessity, as the infantry of the Chinese was powerless against the inroads of those swift horsemen. This measure, further, necessitated on the part of the Chinese the adoption of the tight-fitting riding-costume of their enemies, together with narrow, long-legged riding-boots, while their own national garment had always been spacious, loose, and flowing, and only sandals and shoes (but no boots) had previously been known to them. This case is instructive in many respects: it affords a good example of how the attire of one group of peoples may suddenly become the property of another, and it shows that even a higher civilized nation may take lessons in clothing itself from peoples regarded as barbarians. Above all we see that at this early date the long-legged boot existed among the roving horsemen of central Asia, and that without any doubt the equestrian practice had called it into being. The wide diffusion of mounted infantry and subsequently cavalry, together with the constant migrations, shiftings, expeditions, and conquests of these ever restless nomads, ultimately led to the distribution of the riding-boot all over Asia. For this reason I have always been convinced that the long boots of the present Chinese, as worn by officials and soldiers, whether made of leather or in connection with ceremonial costume of satin or velvet, and further the boots of the Tibetans, Mongols, and Amur tribes, are traceable to that one primeval type. I am therefore somewhat suspicious of what Hatt styles in chapter III "transformed moccasins." If, as he states (p. 229), "the moccasin-boot may undergo further transformation and thereby lose all of its moccasin character," the question may justly be raised whether this theory is necessary, and whether it would not be more plausible to dissociate this type from the moccasin group altogether and simply to regard it as a descendant of the riding-boot. The association of the latter with the horse which I am inclined to assume meets its parallel in the correlation of the moccasin with the snowshoe disclosed by Hatt, and this impresses me as the most important point brought out by his investigation. Unfortunately we still know too little about the history of the snowshoe in order to reach a positive result (some historical data on snowshoes will be found in my article on the Reindeer which is soon to appear). The rationalistic evolution traced by Hatt (p. 243) does not satisfy me.

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[1] It is regrettable that the author has not added a map illustrating the geographical distribution of the various types of moccasin, and that he has not tabulated in convenient form his twenty-one American series which it is not quite easy to remember in detail. Another omission is that the publications of the Jesup Expedition, L. von Schrenck's classical work on the Amur peoples, and Russian literature on the ethnography of Siberia, have not been utilized.

In the general theory summed up by the author at the conclusion of his study we miss again to some degree historical insight. He attempts at reconstructing historical movements from the data of the geographical distribution of a single ethnographical phenomenon, and while this effort may be justifiable for North America, it is apt to invite disaster in Asia where history exists and must be consulted. The author ought to have pursued the problem as to how his deductions stand in relation with the ethnic conditions of northern Asia in ancient times. At all events it is manifest that we cannot (and need not) reconstruct from present-day observations a history for those Asiatic peoples whose history is perfectly well known. Hatt's ultimate theory culminates in the assumption of two large cultural waves which "in prehistoric times" a (rather vague determination) swept over the northern regions.

> The oldest of these, now most fully represented and highest developed in the culture of the Eskimo tribes, did not have snowshoes and therefore could not conquer the vast inland areas; it must have followed and taken into possession the rivers and coasts, and we would call this first great wave the coast culture. The younger culture wave is found fullest and most unmixed in the culture of the Tungusians, although its influence is felt from Lapland to Labrador; it still

has the character of an inland culture and must have originated as such. Its most valuable possession is the snowshoe, which has carried it over the greater part of the Arctic.

At the outset this reconstruction appears tempting, and as the author claims, may prove of some value as a working hypothesis. Yet it seems to me to be subject to some caution. Hatt does not define what he understands by Tungusians. The term "Tungusian," however, comprises two different things. There is a group of scattered clans, widely distributed over northeastern Siberia from the upper Yenisei as far as the Okhotsk Sea and claiming for themselves the name Tongús. These are Tungusians proper. This designation, further, has been extended by our science to a large number of compact tribes massed in the Amur region and Manchuria. These are Tungusians in the wider sense, and their grouping with the Tongús of northeastern Siberia, first of all, is a scientific theory. The history of the entire stock is well known and can be traced in detail from the Chinese annals for several millenniums.[1] Certain it is that the original home of the family was located in Manchuria, and that Tungusians are newcomers and late arrivals in Siberia. From oldest times they were tribes of horsemen, and did not possess any such characteristics that we find at present among the Tungusians of Siberia. These adopted the reindeer culture with all its appurtenances from those Siberian peoples whose territory they occupied. The culture of Siberian Tungusians is very far from exhibiting any originality, and does not occupy the place which Hatt is inclined to assign to it. At any rate I am under the impression that in Hatt's "prehistoric times" there were no Tungusians in Siberia, and that his so-called Tungusian culture did not then exist. There is no reason whatever to argue that the ethnical conditions of northern Asia were the same anciently as at present, while Hatt evidently takes this as a foregone conclusion. Not only the Tungusians, but also the Yakut and Samoyed are but new and recent guests of Siberia, and some of the so-called Palaeo-Asiatic peoples formerly extended much farther to the south. It is hoped that Dr. Hatt will see fit to revise his theories somewhat in the light of historical facts; also if he will take up the history of the Lapp in connection with that of the Samoyed, northern and southern, he will doubtless recognize the interrelations of the Lapp with Siberian groups and be able to place on a more solid basis his assumption that "the Lapp moccasin and the Amur moccasin have sprung from the same source" (p. 238). Whether the oldest moccasin forms originated "somewhere" in northern Asia and spread thence to America is a theory which must be left for the discussion of the Americanists. I must confess, however, that it does not convince me.[1]

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[1] The reader may be referred for a general account to E. H. Parker's *A Thousand Years of the Tartars* (Shanghai, 1895).

B. LAUFER

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COMMENTS ON "THE PLACE OF COILED WARE IN SOUTHWESTERN POTTERY"

IN his article on "The Place of Coiled Ware in Southwestern Pottery"[2] Mr. Morris suggests certain points in the technique which are deserving of somewhat more extended notice. The question of how the vessel was held and how manipulated, arises, because if the pottery as a feature is worth study, surely the way in which it was made may have importance, and is of interest. The "spiral coil which begins at the bottom and proceeds continuously to the rim" is so often laid in one direction as to make the other seem exceptional. From the examination of many jars of various shapes, and from many fragments, it seems probable that the clay ribbon was somewhat flattened, that after the start of the coil on the center of the bottom of the jar, the jar was held in the left hand, fingers inside, thumb outside, the manipulation of the coil done with the right hand, the pressure with the right hand thumb making the pattern as the pressure was applied to make the coil adhere. The jar must have been held with the outside toward the worker, the coil being applied from left to right on the near side of the jar, with the pattern always in view of the worker. This direction of the spiral may be traced whenever the bottom of the jar is unbroken, and may often be seen along the spiral at places where the clay ribbon had to be pieced out and the new length is laid over the end of the old. If the spiral be complete, the ending at the rim will show plainly the direction of the coil. Under a magnifying glass the impression of the skin of the thumb is clearly seen; and one may fit one's thumb into the impression in one of the several ways in which the thumb was held when pressing down the coil and sometimes pushing back the clay to get the desired pattern. Many patterns were worked out by using one or the other side of the end of the thumb, and by pressing upward or downward a distinct "ripple" was worked; sometimes the exactness of position of these points of pressure, one directly over that in the preceding row, on the right of the mark in the last row—more seldom on the left—made the pattern, and very delicate are some of these. On some jars the ribbon was quite narrow, the thumb pressures quite close, but the work was marvelously regular and the pattern clear. That the work was done by hand in most cases and the pattern made by a "tool" less often, is clear from the fact that the edges of the coils are still unbroken, and the pattern worked with the coiling rather than afterward. In many of these finer jars there is no blurring of the coils at any point, even on the base, and it almost seems as if the jar had dried before being set down. In very few jars of the coiled ware, even the large cooking pots, does it seem that a base was made and partly dried before the walls were applied. The larger jars show a blurring of the edges of the coils on the base which may indicate the resting on some surface which could turn easily for the coiling of the walls and neck of the jars.

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[1] The spelling of tribal and geographical names in Hatt's paper calls for some criticism. Aino is wrong for Ainu—an error already rectified a generation ago. Instead of Golds (p. 211) read Gold or Golde; instead of Giljaks (p. 212 and *passim*) read Gilyak; instead of Wiljuisk (p. 231) read Wilui or Wil'ui. The plural form of tribal names is arbitrarily formed with or without *s*: on p. 232, for instance, we have "the Ostyak, Vogul, and Samoyed," but eleven lines below "the Lapps." He who spells Sakhalin, as has been done here, must be consistent in writing Okhotsk (not Ochotsk, p. 232) and Turukhansk (not Turuchansk, p. 231). Orotchon (p. 229) is redundant; phonetically the question is merely of a palatal explosive surd (Oročon), so that in English spelling Orochon is sufficient, or at best Orotshon would be admissible. Finally, the spelling Chukchee for Chukchi is a barbarism; then, why not spell Asqueemoe?—Is it unknown to the editors of our Memoirs that the name Field Columbian Museum (pp. 149 and 185), for which the abbreviation F. C. M. is especially invented, has been abolished since November 1905?

[2] *American Anthropologist* (N. S.), vol. 19, no. 1 (Jan.–Mar., 1917), p. 24.

The manner of holding the jar with the pattern always in view seems reasonable, because the work could hardly have been done with so much accuracy if the decoration were on the far side, out of sight. And it would be so awkward to turn the jar over with each stroke.

The fact that spiral coiled pottery is no longer made among North American Indians leaves us uncertain of the exact manner of making. But even in other areas where the spiral is now in vogue, the reports are not detailed enough to indicate the method. That the jar is "held in one hand and worked with the other" is as close as we come to the way of making in some accounts. So also in spiral basket making we are sometimes told that the "stitch goes over the coil"; but which way? Is the stitch drawn through under the coil toward the worker, or does the stitch follow the awl-hole, pushed through under the coil and pulled away from the worker? It would be so helpful in attempting to reconstruct a technique, or even to visualize it, if the precise manner of manipulation could be stated: just the way the object should be held, and what is done with the left hand and what with the right hand. Many striking correspondences appear between different techniques in details of holding and handling—as between coiled pottery and coiled basket work—and sometimes the question of the usual direction of a coil is raised, as, for instance, that in the formation of the Eskimo winter house. Another question suggested is that of the habits in hand work among men and among women, whether the manner of holding the work would be the same, and the manipulating, or whether it would be different—as is sometimes the case in sewing. Perhaps more detailed information on each technique would bring out these resemblances or establish the differences.

AGNES C. L. DONOHUGH

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"THE DISTRIBUTION OF THE METHODS OF FIRE-MAKING":[1] A REPLY

DR. DIXON has recently called attention to some discrepancies in my paper on migration[2] and his kindly interest is much appreciated. He is quite right in making much over my inadvertent error in stating that the flexible thong method is confined to Borneo, and apologies are due Henry Balfour for the unintentional statement which will be corrected in a subsequent publication. Dr. Dixon has not contributed any facts that militate against the characterization of Africa and America as areas of the fire drill. It is also believed that this is the substratum method in Australia, as the drill was used by tribes farthest from the influences that came through the York peninsula. Manifestly, since the paper was not a discussion of the distribution of fire-making methods, it was unnecessary to confine the plough to Polynesians whose only method it is, or to trace the plough over the vast confines of Melanesia. Not much stress is laid upon the suggestions as to the distribution of prehistoric peoples to the mainland, or their identification with existing tribes formerly thought to resemble oceanic tribes. It may be said that one group of the Malays are so distributed, but in a comparatively limited area. As to the Philippines the language drift shows Malayo-Polynesian influence reaching these islands.

WALTER HOUGH

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[1] *American Anthropologist*, vol. 18, no. 3, p. 445.

[2] "The Distribution of Man in Relation to the Invention of Fire-Making Methods," *American Anthropologist* (N. S.), vol. 18, no. 2.

PROCEEDINGS OF THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

Meeting of October 17, 1916

THE 500th meeting of the Society was held at the New National Museum, October 17, 1916, at 4:30 p.m. Dr. Walter Hough, of the U. S. National Museum, read a paper on "Ancient Pit Dwellers of New Mexico."

Dr. Hough said that remains of pit dwellings were indicated on the site of stone ruins explored in western Socorro county, New Mexico, several years ago and that a large cluster of such dwellings not connected with stone ruins was subsequently observed near Luna in Socorro county. This site was productive of interesting results during an exploration for the Bureau of American Ethnology last summer. The field in which the remains occur had been smoothed over by natural agencies and the positions of the houses were shown only by the stronger growth of vegetation over circular areas, thus defining the pits. Some of the pits were cleared and it was found that they were from twelve to fourteen feet in diameter and five and one-half feet deep. Remains of roof clay and charred posts and beams indicate that the roof was supported on posts placed around the periphery of the pit. It is thus probable that more than half of the house was underground and perhaps the portions of the walls above ground were banked. The floor arrangement shows a fireplace near the center, a metate and grinding stones near the fire. In one of the pits a burnt clay wall fireplace was found. Adjoining the pits was a rectangular house also roofed with clay. Here were found numerous grinding stones, baking slabs, and remains of pottery, these being about a foot under the surface loam. This house was an open air cooking and mealing shed. Near the first pit excavated was a cemetery of infants; no remains of adults have yet been found on the site.

A dance amphitheater which was about one hundred feet in diameter and ten feet deep lies on the east side of the site. It has been filled with about five feet of fire-black debris by natural agencies and in it large pine trees have matured. It is on the bank of the former and larger channel of a living stream which traverses the eastern edge of the site. The specimens found are crude metates, rubbing stones, hammerstones, baking slabs, etc.; pottery of Pueblo type decorated and undecorated and of some crudity; a few bone awls; and small obsidian arrowheads. No stone axes were found. Dr. Hough presented the problem as he found it and hesitated as yet to pronounce upon the affiliations of the people who constructed the pit houses.

Meeting of November 7, 1916

The 501st meeting of the Society was held in the Lecture Hall of the Public Library, on Tuesday evening, November 7, 1916, at 8 o'clock. The speaker was Dr. W. E. Safford, of the Bureau of Plant Industry, U. S. Department of Agriculture. His subject was "Magic Plants of the Ancient Americans," illustrated with lantern slides.

Dr. Safford said that the practice of magic was widely spread in both North and South America in pre-columbian times, and in connection with it certain plants, principally those having narcotic properties, were used ceremonially, often as incense, or to produce hallucinations, call up the spirits of the dead, and to expel evil spirits from the sick and insane. The priests of the Temple of the Sun at Sagomozo, in the Andes of South America, prophesied and revealed hidden treasures, while in a state of frenzy caused by the seeds of a tree-datura (*Brugamansia sanguinea*), recalling similar practices of the priestesses of the oracle at Delphi. Another Peruvian plant with marvelous properties described by early explorers was *Erythroxylon Coca*, from which the valuable alkaloid cocaine is now obtained. Bags of its leaves accompanied by little gourds containing lime were found by the author in many graves near the Peruvian coast, hanging about the necks of the mummified remains of the dead. On the opposite coast of South America, or rather in Paraguay, grew the highly esteemed *Ilex paraguariensis*, or *yerba mate*. Closely allied to it is the *Ilex vomitoria* of the southeastern United States, from which the Indians made the famous "black drink," used ceremonially as a magic physic, which purged them from evil and which was also used in initiating their youths into manhood. Professional priests, or necromancers, encountered by Columbus and his companions on the island of Hispaniola, induced intoxication and called up their *zemi*, or gods, by means of a narcotic snuff, called *cohoba*, inhaled through the nostrils by means of a bifurcated tube. This snuff, hitherto believed to have been tobacco, has been recently identified by the author as the powdered seeds of a Mimosa-like tree, *Piptadenia peregrina*, still used in a similar way by various South American tribes of Indians, by some of whom an infusion of the seeds is also used to induce intoxication administered as an enema by means of a pear-shaped syringe of caoutchouc. In Mexico the early missionaries, who were called upon to stamp out the practice of witchcraft, found that the Aztecs paid divine honors to various plants, especially to *Huauhtli* (a white-seeded *Amaranthus*): *Ololiuhqui* (a *Datura*), *Peyotl* (a spineless cactus, *Lophophora Williamsii*) also called *Teonanacatl*, or "Sacred Mushroom"; and *Picietl* (Tobacco). Of *Huauhtli* seeds ground to a paste with the syrup of Maguey images were made and adored, and afterwards broken into fragments and served as a kind of communion. This seed was produced in such quantities that it was used in paying tribute to Montezuma, at the time of the Conquest. The *Ololiuhqui* was regarded as divine, and it was considered a holy task to sweep the ground where it grew. Its spirit addressed as the Green Woman (*Xoxouhqui Cihuatl*), was invoked to expel certain diseases and to overcome weaker and inferior spirits in possession of a sick person. It is interesting to note that the use of the *Ololiuhqui*, or *Toloatzin*, as it was also called (*Datura meteloides*) still prevails among the Zuñi Indians of New Mexico, the Paiutes, and several tribes of southern California, in certain religious and ceremonial practices, especially in initiating youths into the status of manhood. The *Peyotl*, or *Teonanacatl*, called by Bancroft the "flesh of the gods," was used by the Aztecs in nocturnal feasts, very much as it is still used by Indians of the Mexican Sierra Madre and by certain tribes of the United States, who believe the visions induced by it to be supernatural. In ancient times a supply of this little narcotic plant was obtained by runners especially consecrated for the purpose, and its gathering was attended by a most formal ceremony. At the present day it is sent from the locality, where it grows, along the Rio Grande, by means of parcel post. Lastly the ceremonial and religious use of *Picietl*, or tobacco, goes back to remote antiquity It is so well known that it need not be here discussed. No other narcotic plant, perhaps, has become so widely spread or so generally used and beloved by its votaries. Though of subtropical origin its cultivation had extended before the Discovery as far north as the St. Lawrence river. Beautiful pipes of many forms, representing birds, mammals, human heads, etc., have been discovered in Indian mounds near the native city of the writer, Chillicothe, Ross county, Ohio; and more recently in the county of Scioto farther to the southward. In addition to the above plants may be mentioned a certain small scarlet bean, the seed of *Sophora secundiflora*, endemic in northern Mexico and southern Texas. This also has narcotic properties, and was so much sought after by certain tribes of Indians that they were known to exchange a pony for a string of the beans six feet in length. In one of the secret societies of the Iowa Indians this bean is used in the initiating ceremonial; and it is interesting to note that the beans are carried as charms or amulets by the members of the society, just as in western Mexico fragments of the *Peyotl*, and in southern California parts of the *Datura*, are carried by their votaries, who believe them to be efficacious in protecting those who carry them from danger, and in bringing good luck in hunting and war. It is interesting to note a similar practice in the Old World of carrying the root of *Mandragora* (or a substitute for it) as an amulet; but most interesting of all is the similarity between the beliefs and practices of the inhabitants of the Old World and New, in connection with narcotic and other plants held to possess magic properties. The lantern slides used to illustrate the lecture were photographs of the various magic plants discussed.

This paper was discussed by Mr. James Mooney who, for a number of years, has given special study to the subject of peyote. Mr. Mooney defended those who are devoted to the peyote ceremony and claimed that, in most cases, the assertions made against the plant and its users are based upon ignorance of the facts, and are without foundation.

Meeting of November 21, 1916

THE 502nd meeting of the Society was held at the New National Museum on Tuesday afternoon, November 21, at 4 o'clock. Mr. Neil M. Judd of the U. S. National Museum presented a paper on "New Types of Pueblo Ruins found in Western Utah," illustrating his paper with chalk drawings.

Mr. Judd described a recent archaeological reconnoissance of western Utah conducted under the auspices of the Bureau of American Ethnology, stating that this resulted in the discovery of two types of prehistoric habitations not previously reported from the Southwest. The first of these was circular in form and was made by leaning logs against cross-pieces supported by four uprights which surrounded a central fire-place. Willows, grass and clay, in succession, covered the logs. Houses of the second type, usually occurring in groups forming villages, were rectangular in shape and constructed entirely of adobe. A small series of unattached cliff-dwellings, exhibiting certain features common both to structures of this second type and to stone-walled houses south and east of the Rio Colorado, was also described. A careful study of the smaller artifacts recovered from both types of western Utah ruins indicates a close cultural relationship between their respective builders and the inhabitants of prehistoric structures in other sections of the Southwest.

In a discussion of the paper Dr. J. W. Fewkes called attention to the desirability of a more accurate definition of what archaeologists mean by a "pueblo." He pointed out that the term is sometimes loosely used to include all kinds of ruined stone buildings in the Southwest. Inasmuch as the pueblo culture area owes its name to characteristic buildings or pueblos he suggested that the term be limited to terraced, congested community buildings with ceremonial rooms or kivas. If this suggestion were accepted by archaeologists many ruins on the periphery of the so-called Pueblo area would have to be classified as belonging to a pre-puebloan phase, or not regarded as pueblos at all.

Meeting of December 5, 1916

THE 503rd meeting of the Society was held in the Lecture Hall of the Public Library on Tuesday evening, December 5, 1916, at 8 o'clock. At this meeting Prof. W. H. Holmes, of the U. S. National Museum, delivered an address on "Outlines of American Aboriginal History," illustrated with lantern slides.

Introducing his subject Prof. Holmes said: It is agreed that the human race is a unit. It follows therefore that there was but one cradle and that from this man spread over the world. The early chapters of human history must always remain obscure although evidence has been found carrying the story far back into the remote past.

It was the purpose of the speaker to indicate briefly the probable course taken by the human race in spreading from the Asiatic cradle to the New World, and also to indicate the causes and course of cultural development in the various centers of American occupancy and to suggest the causes of decline.

The earliest known traces of man (or a man-like being) have been found on the island of Java. In the nature of things, it was a long time before he wandered far from his primeval home. He had to acquire the arts of the hunter and fisher before he could reach the far north and it was doubtless by way of Bering strait that he reached the New World. Portraits were shown of the various peoples whose ancestors may have been concerned in these ancient migrations—natives of Tibet, China and Siberia, the Eskimo, the Sioux, the Zuñi, and other typical American Indians; and attention was directed to the practical identity of these types. Referring to the development of culture in America it was shown that no culture above the hunter-fisher stage ever passed through the Bering gateway. All culture of higher grade is, therefore, American. As the early immigrants reached the more favorable localities of the continent they engaged in agriculture and became sedentary. This condition led to the development of the simple arts, industries and institutions belonging to this phase of progress. It was asked how then do we account for the vast works in the way of temples, pyramids, and tombs and the vast body of products of the sculptor's art, of highly embellished pottery, textiles and metallurgy? It was shown that all were due to the dynamic forces of religion manifested through the all-powerful shamanistic classes who sought above all things to honor the gods and to glorify themselves. But it is asked why are the once splendid cities now in ruin? The reasons are readily found. The energies of the people were broken down by the ever-increasing load of super-essential activities. The system which permitted overgrowth of these highest manifestations of culture had within itself the germs of disaster.

Numerous slides illustrating various wonderful products of sculptural and architectural genius developed under the absolute control of the Mayan shamanistic priesthood were shown.

The paper was discussed by Dr. E. Hewett of Santa Fe, New Mexico; Mr. S. G. Morley of Cambridge, Massachusetts and Dr. H. J. Spinden of New York. Dr. Hewett suggested the influence of environment on the cultural development of a race, citing the discovery of metal by the early inhabitants of Europe and its use in making weapons. Mr. Morley spoke of the Maya hieroglyphs, and Dr. Spinden discussed the culture which preceded that of the Maya in Central America, traces of the agricultural period remaining in crude figurines and that of the religious period in effigies of serpents and of grotesque gods.

Meeting of December 19, 1916

The 504th meeting of the Society was held at the New National Museum, December 19, 1916, at 4:30 p.m. Dr. Truman Michelson of the Bureau of American Ethnology presented a paper entitled "Notes on the Peoria Indians."

Dr. Michelson said that the Peoria Indians have practically lost their ethnology in the strictest sense of the word, although their language and folklore still remain. A study of these show very clearly that the Peoria Indians have had two associations, the older and more intimate association with the Ojibwa group of central Algonkians, and the more recent with the Sauk, Fox and Kickapoo. The terms of relationship support this view, as do the historical facts.

The speaker then showed by means of a blackboard chart the system of consanguinity among the Peoria. In this way it was demonstrated that two factors were involved, *i. e.*, the gentile organization and blood consanguinity.

The paper was followed by a general and interesting discussion. Prof. W. H. Holmes mentioned the old quarries in northeastern Oklahoma, near the present location of the Peoria Indians, and Dr. Aleš Hrdlička noted the importance of definite recording of the geographic distribution of Indian tribes. In reply to a question by Mr. F. W. Hodge the speaker expressed the opinion that archaeological research would throw light on the early material culture of the Peoria. Dr. J. R. Swanton noted that the Indians of the northwest coast have a myth similar to one related by the speaker in which bones thrown into the water are said to "come to life." In the legend of the coast Indians the bones are those of a salmon, while among the Peoria the bones are those of a beaver. In response Dr. Michelson stated that the form of this legend which mentions the beaver is limited to a small area.

Meeting of January 16, 1917

THE 505th regular meeting of the Anthropological Society of Washington was held January 16. At this meeting Mr. William H. Babcock presented a paper on "Certain Pre-Columbian Notices of the Inhabitants of the Atlantic Islands."

Beginning at the north, the speaker referred to the well known Norse colonization of Iceland in the latter part of the ninth century and the much earlier establishment of Irish monks at some points of its coast. From numerous Celtic local names Dr. Nansen argues that there may have been a much greater inland Irish settlement; but perhaps these names should rather be explained by the many Irish and Hebrideans among the early Norse colonists. There are also mentions of "trolls" in the sagar of Grettir—and perhaps elsewhere in early Norse writings—as inhabiting hidden places of the mountainous interior. These may preserve possibly the memory of aboriginal survivals from Pre-Norse and Pre-Irish times; but more probably merely echo old Norse traditions or are freaks of fancy.

Farther south, perhaps the oldest record is Plato's Atlantis derived from his ancestor Solon who received it, as we are told, from the Egyptian priests of Sais. The speaker related it in slightly condensed form with special heed to anthropologic items: but expressed the opinion that it merely reflects the civilized life of Plato's time on the Mediterranean shores: and throws no light on mankind in the Atlantic.

He recited also the description of a far western island, which seems to be Madeira, given in the time of Julius Caesar by Diodorus Siculus: adding the same estimate.

He held that this also applies to Plutarch's interesting account of Ogyggia, possibly Ireland or Iceland and the continent beyond it; also to the Irish sea romances or Imrama, though the home life which they at times repeated in their tales was very different from the life of the Mediterranean.

In the Geography of the Arab writer Edrisi—about 1154—we seem to find, perhaps for the first time, notes of observations of real human beings on the islands of the eastern Atlantic. A list of these was given with most of the relevant items; showing great diversity in matters of culture, perhaps also of race, but in part agreeing very well with fourteenth and fifteenth century accounts of the Canary Islanders. His sources were partly mythical, partly recent Arabic and to some extent probably also European, both classic and mediaeval. It is impracticable to identify each island; but some of them are located among the Canaries; one is perhaps Madeira, and two or three at least should be credited to the Azores.

We have nothing certain and explicit concerning the latter after this time until the Portuguese colonization, which seems to have found no one in possession: but fortunately there is a considerable body of information as to the Canary Islanders.

The speaker quoted from Major's introduction to the Canarien of Bontier and Leverrier a translation of a letter written at the end of 1341 by certain Florentine merchants dwelling in Seville, Spain, narrating an expedition that year of three Portuguese ships, partly manned by Italian seamen, to the Canary Islands. It constitutes one of the best reports of their people ever made and at that time they were less affected by European interference than afterward.

After mentioning a brief settlement of thirteen Spaniards in Grand Canary in 1382 and the beginning in 1402 by Bethencourt of the conquest of the islands, completed about 1485, Mr. Babcock, from the same source (Major's introduction) quoted Azurara's narrative (in the *History of the Conquest of Guinea*, published 1448) of a slave raid on the island of Palma, participated in by a Portuguese vessel in 1443, with some account of the inhabitants of the other islands, also Cadamosto's report on the Canary Islands in 1455, the middle of the contest, some being still unconquered and pagan. He added from the body of the Canarien divers selections presenting similar matter of anthropology.

A few words as to matters of race and culture closed the paper.

The presentation of the paper was followed by a general discussion of the subject in which the speakers were Dr. Aleš Hrdlička, Dr. John R. Swanton and Dr. Truman Michelson.

Meeting of February 6, 1917

The 506th meeting of the Anthropological Society of Washington was held in the Lecture Hall of the Public Library, Tuesday evening February 6, 1917, at 8 o'clock.

Dr. J. Walter Fewkes, of the Bureau of American Ethnology, read a paper on "Prehistoric Ruins of the Mesa Verde National Park." This communication was mainly taken up by an outline of the work accomplished by him, in the summer of 1916, under direction of the Bureau of American Ethnology, at the request of the Department of the Interior. After a brief introduction on the situation and physical features of the Park and a short account of archaeological work already accomplished, the speaker described in detail the uncovering and repair of one of the large pueblo-like buildings of the Mummy lake group, situated on the government road, four miles and a half from the ruin known as Spruce-tree House.

The mound excavated is one of the largest of the group, and when the work began gave no indication of the form, size, and architectural features of the building it covered. After three months' work there was brought to light a rectangular structure, 113 feet long by 100 feet wide; three stories high, with an enclosed court on the south side. The ground plan showed the existence of four circular, ceremonial rooms completely embedded in fifty rectangular enclosures which were formerly used for secular purposes. The remarkable feature of this ruin is the large size of one of the circular rooms, situated in the center of a compact group of chambers. From the wide southerly outlook this ruin has received the name, Far View House. It is a pueblo habitation, the first of its type ever brought to light on the plateau. The ruin was repaired, the tops of the kivas being treated with Portland cement to protect them from the elements.

After describing the various architectural details of the building Doctor Fewkes passed to a consideration of what he termed the morphology of the structure, or the comparison of it with other types, especially the cliff dwellings of the Mesa Verde. He declared that it is a new type of ruin in that region, and that there are evidences of many other examples of the same general character now indicated by mounds; we may say that formerly there were as many members of this type on the Park as cliff dwellings in the caves of the canyons. He considered in detail some of the arguments bearing on the relative age of buildings like Far View House, and the cliff dwellings, and came to the conclusion that the former were the more recent, and evolved from the habitations in cliffs.

Considerable time was devoted to a discussion and comparison of the so-called kiva or sacred room. He held that this chamber should be made the basis of classification of pueblo ruins, and that it was represented by the tower found widely distributed in Utah, and adjacent regions of Colorado. He pointed out the wide-spread custom of dual styles of buildings among primitive races: one type being devoted to religious purposes, the other to habitations. He claimed that the former are always better constructed than the latter. He regarded the tower as a religious building and that the people who used it lived in dugouts or temporary habitations that have disappeared. In the earliest times these two types were separated, but in later stages in the evolution of buildings they became united; at this time the habitations were constructed around the bases of the towers. Later in the course of development the central original building lost its tower-like form and became the circular kiva. Several similar architectural units, by union, formed a pueblo.

He claimed that all the modern pueblos show indications of modifications due to foreign influences, especially Mexican, but that Far View House, with its union of kiva and other rooms was an unmodified extinct type.

Doctor Fewkes pointed out that the great morphological similarity between Far View House and the pueblos with central kivas and towers, many miles away, had an important bearing on the distribution or diffusion of pueblo culture. He regarded the San Juan region as the nucleus from which the pueblos south and west originated, thus substantiating by archaeological evidence the legendary traditions of the inhabited and much modified historic pueblos. He claimed that there were two nuclei of distribution of house builders in the southwest, each arising in regions physiographically and climatically distinct; each possessed of different materials available for architectural advancement: one arose in the Gila valley; the other in the San Juan; the former spread toward the north, the latter to the south. Both nuclei were extinct before the historic epoch. What remained, or that we now know as the culture of living descendents is the product of acculturation, due to cultural contacts in this expansion. History can afford, therefore, only an imperfect picture; we must rely on archaeology, mainly architectural and ceramic remains, supplemented by ethnology, to discover the nature of the culture of these two original nuclei.

In a discussion of their distribution the speaker showed numerous illustrations of the prehistoric kivas called towers, situated in Hill canyon, near Ouray, Utah. To these he gave the name, suggested by their site, Mushroom Rock ruins; their more striking peculiarity being their position on tops of inverted cones, or mushroom-like formations of rock due to the enormous erosion evident in the region where they occur. He said that this form was not morphologically a different type from towers, but its site was so unusual that it was convenient to designate them by this name.

While the important question of the antiquity of the cliff dwellings has not been satisfactorily answered by the observations made at Far View House, progress is being made in the accumulation of significant data bearing upon it, and that as long as this question remains unanswered the archaeologist has plenty of research before him for many more years of fieldwork in the Southwest.

The communication was illustrated with lantern slides.

Meeting of February 20, 1917

The 507th meeting of the Anthropological Society of Washington was held in Room 44 of the New National Museum, February 20, at 4 p.m. The speaker of the afternoon was Dr. I. M. Casanowicz of the New National Museum who presented a paper on "The Fish in Cult, Myth, and Symbol."

Dr. Casanowicz said:

> The fish, as the inhabitant of the mysterious, indestructible never-resting water, early impressed man deeply, and was considered by him as the genius and representative of the life-producing element. Traces of the veneration of the fish, sometimes revealed in taboos, are found everywhere in ancient times and still exist in various parts of the world.

A center of ichthyolatry in antiquity was, according to the testimony of classical writers, especially Syria, where a fish goddess under the name of Derketo-Atargatios was worshiped as a phase of the great Semitic mother goddess Astarte, being regarded as a personification of the fructifying power of the water. Reminiscences of this cult still survive in the cherishing of sacred inviolate fishes in some places near mosques.

Tales of the fish as a medium of transformation and incarnation of spirits and ghosts are met with among various nations, and in later times the fish seems to have been, next to the bird, a symbol of the departed human soul. The fish as carrier of man across the water was illustrated by the story of Arion and the dolphin as told by Herodotus, and by the Biblical narrative contained in the book of Jonah. Parallel narratives of a man being swallowed by a sea monster were quoted from Greek, Polynesian and Cherokee lore.

The belief in the magical healing and apotropaic properties of the fish were also found to be wide-spread. The fish was generally considered as a being of good omen, benevolent and beneficent to man, and by reason of its own great fertility it was a symbol of increase and abundance. Various regions had their favorite species of fish which were endowed with supernatural qualities. Thus among the classical nations the dolphin was termed the "saviour fish" (*piscis salvator*). In the Far East (China and Japan) the carp was the fish of good omen, while among the ancient Irish the salmon was the "fish of wisdom," the mere sight of which brought healing.

Dr. Swanton introduced the discussion of the paper by calling attention to the fact that migrations of Indians were influenced largely by the food supply. Thus many tribes of Indians followed the rivers and streams because of the presence of fish. Dr. Michelson mentioned the legend of a miraculous fish among the Delaware Indians, and also noted a similarity between the ancient beliefs concerning the fish and those held by the North American Indians. Mr. E. T. Williams spoke on the use of the fish in Japan and China, the latter country considering it martial in character because of its fearlessness in proceeding against the current of a stream, and also because of its scales, suggesting a coat of mail.

Meeting of March 6, 1917

THE 508th meeting of the Anthropological Society of Washington was held at the New National Museum on March 6. This meeting was devoted to a general discussion, the subject being "Problems Connected with the Distribution of the Aboriginal Population of America." The speakers were Dr. John R. Swanton, Dr. Aleš Hrdlička, Dr. Truman Michelson, Prof. William H. Holmes, Dr. J. Walter Fewkes, Dr. Walter Hough, and Mr. Francis LaFlesche.

Dr. Swanton introduced the discussion by stating that the subject divided itself into a consideration of the distribution of aboriginal population in America quantitatively and qualitatively. Dr. Swanton said:

> Populations may be classified qualitatively according to their physical characteristics, languages, cultural features, social organization, and so on. Archaeology has a bearing on all these.

He gave as one of the principal problems to be considered the bearing of the data of each class on the generally admitted Asiatic origin of the American Indians and their diffusion from the northwest.

Dr. Aleš Hrdlička, speaking from the standpoint of physical anthropology, stated that the distribution of different physical types on the American continent has always been one of the main problems of his branch of science in this country. Morton, the father of American anthropology, classified the American Indians in two types, (1) the Toltec, or refined type, which included the Toltecs of Mexico, the Maya, and the mound-builders of the Ohio valley; and (2) the Barbarian, in which he included all the less civilized, semi-nomadic tribes.

This classification, although imperfect, was shown in the course of time to have a good foundation. Roughly speaking we recognize today two great sub-types of the American aborigines, which correspond in the main to Morton's groups. The Toltec strain shows an irregular but wide distribution over both Americas. Its main areas are portions of the northwest coast, a part of the Pueblo region, a large part of the more southern territory of the mound-builders, all Yucatan, Southern Mexico, Central America, the Antilles, the western region of northern South America, and the coast of Peru, with as yet undetermined areas in Brazil, and traces even farther south. The type is principally marked by brachycephaly. The second Morton group corresponds to the American dolichocephalic population which extends over vast areas from Labrador and Canada to Tierra del Fuego. Which of these two types is the older on the Continent has not yet been determined. The answer will doubtless differ in different localities. Besides these two, what may be called fundamental physical types of the American population, we now recognize a third group which, though closely related to the first or Toltec, seems of much more recent introduction and development; this is the Athapascan. And there is a fourth type also of fairly recent introduction, namely the Eskimo. Outside of these four strains, all of which are related and proceed probably from one ancient stratum, we have discovered as yet in America no trace of any other Pre-Columbian population.

Dr. Truman Michelson, speaking on the linguistics of the Indians, said: "There is no single type of language, no fundamental structure that is the same in all linguistic stocks, though we find resemblances among them." The speaker stated that resemblances occur between the languages of northeastern Asia and those of certain North American Indians. "An important problem in linguistics," said Dr. Michelson, "is to determine whether resemblances between languages are genetic or borrowed." The distribution of linguistic stocks was indicated on maps.

Professor Wm. H. Holmes spoke briefly of the probable origin of the human race in southern Asia and the gradual spread from the cradle through increase in numbers and intelligence over wide areas. In passing northward, the culture would be gradually modified and on reaching the Arctic, it would be reduced to a state corresponding to that of the hunter-fisher state exclusively known throughout the Arctic. In passing by the Bering route to America migrating groups would carry with them only this single culture stage, but advancing southward, changes would take place according to environment. Culture would take on one phase in the Great Plains region, another in the Mississippi valley, still another in Mexico, and so on; and there would follow interchanges of culture elements between peoples and areas without end. We thus explain the complex conditions and great diversity of the Columbian period.

Dr. J. Walter Fewkes stated that the two great forces which have influenced the distribution of population in North and South America are (1) geographic, the course of migration being somewhat determined by the mountain ranges and rivers, and (2) the food supply, which depended on the climate. Dr. Fewkes called attention to the fact that language does not represent the cultural distribution of a people. Languages shrink and change but archaeological evidence represents culture in its original distribution and affords a permanent basis of study. Thus archaeology indicates that two types of people once lived in the southwest. There were two foci of distribution, one in the San Juan valley and the other in the Gila valley. The food quest broadened the outlines of these groups, and at the point of juncture there arose a mixed type, which we now find along the Little Colorado, while in the original places the culture has entirely disappeared. Dr. Fewkes spoke of the large and important cultural groups found in the West Indies where the Carib stock was preceded by another stock, the Arawak, both originating in South America. In the West Indies the root of the yucca bears the same relation to the food problem of the people that the corn bears in the southwest.

Dr. Hough said that at the time of the Discovery the tribes of America were identified with the environments in which they had settled and since that time only migrations of a minor character had taken place. The causes of extensive migration were thus conjectural but depended on basic facts of food, transportation and artificial fire-making.

Mr. La Flesche stated that the ancient rites of the Siouan stock show that the migrations of the people were influenced by the search for food. The first animal mentioned in these rites is the elk, succeeded by the deer and later by the buffalo, at which period the mention of corn appears for the first time.

Meeting of March 19, 1917

THE 509th meeting of the Anthropological Society of Washington was held in the Lecture Hall of the Public Library on March 19, 1917. On this occasion Dr. Fay Cooper Cole, of the Field Museum of Natural History, Chicago, Ill., delivered a lecture before the society on "The Pagan Tribes of the Philippines." The lecture was illustrated by lantern slides.

Meeting of April 3, 1917

THE 510th meeting of the Anthropological Society of Washington was held at the New National Museum, April 3, 1917. At this meeting Dr. Leo J. Frachtenberg, of the Bureau of American Ethnology, Smithsonian Institution, Washington, D. C., presented a paper on "The Religious Ideas of the Northwest Coast Indians."

Dr. Frachtenberg stated that four important features of the religious ideas noted among the Indians of this region are (1) an intensive animism; (2) a belief in the powers of supernatural beings, as dwarfs and giants; (3) a belief in the existence of guardian spirits, and (4) a complete absence of the social phase of religion.

According to Dr. Frachtenberg many religious ideas are common to all the tribes of the northwest coast, yet the northern and southern portions of this area are found to differ in cosmogony. The tribes in the extreme southern portion believe that the world was created out of a watery mist, the Transformer enlarging a small piece of land until it became large enough for habitation. The tribes of the northern portion are satisfied with a world whose origin is not explained. They hold, however, that the Transformer (Creator) first made man and the members of faunal and floral kingdoms, and at a later time returned and improved this creation. Two visits of the Transformer are typical of this region. In the south the Transformer (Creator) and the Trickster are separate individuals, while in the north they are unified. In the south the Transformer makes all that is good, and the Trickster is held responsible for all the bad elements, while in the north there is no such disassociation. Good and evil things were alike created by the Transformer.

The northwest coast Indians believe that an individual comprises a body inhabited by two souls and a ghost. In a slight illness the outer soul becomes separated from the body, in a serious illness the inner soul wanders to the country of souls but may be recalled by the shaman. When death occurs the ghost also departs and the shaman has no further power. The country of souls contains two divisions, one inhabited by recently arrived souls and the other by souls which have been there for a longer time. On their journey to this land the souls pass a rest-house, then a lake and a berry-ground. Their way is barred by a spring-pole, and they must cross a rotten log, the final barrier being a wide river.

No ritual or systematic form of supplication is found among these Indians, indeed it may be said that guardian spirits take the place of deities. Every man and woman possesses one or more guardian spirits, each of which has its special sphere of influence. Thus there are guardian spirits for securing good weather and for plenty of seal or whale, guardian spirits for success in hunting, and for help in making baskets and canoes. No offerings accompany a request to a guardian spirit. Crude representations of these spirits are seen on the implements and on the house-posts of their owners.

The shamans receive their power from a multitude of spirits. Certain shamans are considered to have power to cure sickness, while it is believed that others can steal a man's soul, causing either serious illness or death. Large gifts are exacted by the shamans, who are both respected and feared.

In the discussion which followed the reading of the paper Dr. J. R. Swanton called attention to the nascent dualism and monism in the religious beliefs of the Indians of the southern and northern areas considered by the speaker. Dr. I. M. Casanowicz noted the strange fact that many primitive religious ideas bear a resemblance to the most advanced religious ideas of the present time. Dr. J. W. Fewkes said that primitive Indians are so low in the cultural scale that they develop only the most general religious principles, which are connected with the several geographic areas inhabited by them. He stated further that there is no unity in the primitive religions of the American Indians, though some parallelisms are found between the beliefs held in different areas. Dr. Truman Michelson mentioned some differences between the religious ideas of the northwest coast Indians and those of the Algonquian tribes, one of the chief differences being that the latter do not believe in a plurality of souls.

Frances Densmore, *Secretary*

ANTHROPOLOGICAL NOTES

PETROGLYPHS ON KODIAK ISLAND, ALASKA

THE accompanying photographs (fig. 53), taken by Captain C. A. Halvorsen, the superintendent of the Alaska Packers Association at Alitak, Alaska, show certain symbols cut in the granite formation at Cape Alitak, which forms the northern entrance point of the bay of that name on the southwest side of Kodiak island, Alaska, in approximate Latitude 56° 50′ 30″ N., Longitude 154° 18′ W.

These photographs may be of interest to students of ethnology, and may lead to a better understanding of an ancient people, or possibly of a former race inhabiting these islands.

Views no. 1 and no. 2 were taken from the end of Cape Alitak at an elevation of about fifty feet above sea level, over which the sea washes in heavy weather so that the surface of the hard granite rock is worn smooth and slippery. The symbols at this point are not very distinct, but they have apparently been cut quite deep in the rock many ages ago. The ledge is badly fissured and many of the original symbols have been destroyed. Those that were more marked were traced with black paint in order that they could be seen in the photograph. There are several hundred of these symbols, but time prevented a complete development of the locality.

The face of the rock lies on an incline of about twenty degrees, necessitating a scaffold to obtain a direct view over the ledge. The day was cloudy, making it difficult to obtain good photographic results.

The ledge from which views no. 3 and no. 4 were taken is located near the beach and about one-half a mile northeasterly from the Cape. Most of these figures are quite distinct, but time has shown its action, and in order to strengthen them in the film, they were traced over with paint. In no. 4 may be seen two figures not traced; in fact, they were not noticed until the film was developed.

The small village of Akiok, on Alitak bay, is the only native settlement in the vicinity. Here was found an old native, about eighty-five years of age, who, when interviewed by the Russian priest, declared that the symbols referred to were ancient carvings of an unknown race, and that neither his father nor his father's father had ever heard any tradi-

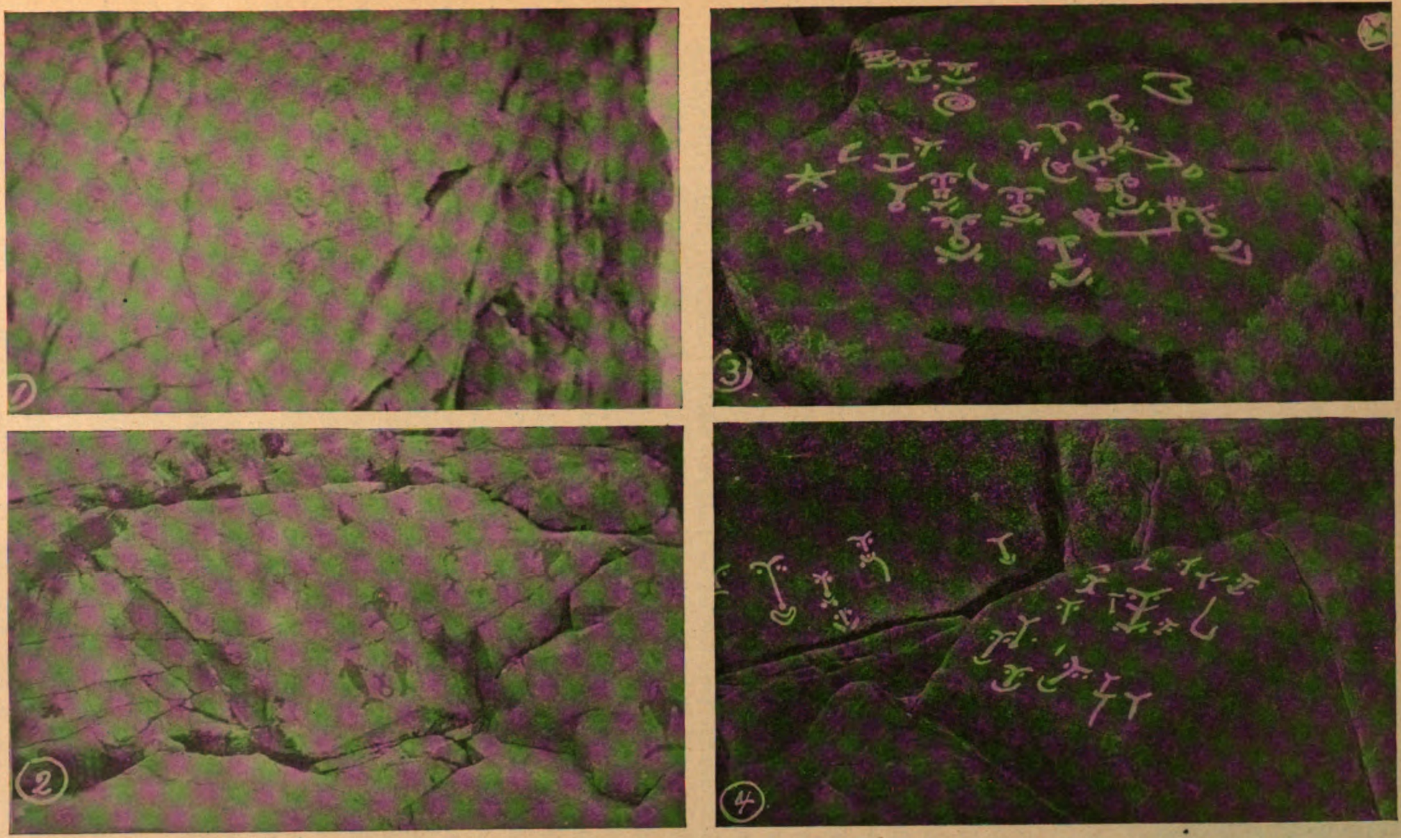

FIG. 53.—Petroglyphs at Cape Alitak, Kodiak Island, Alaska.

tions connected with them. The general belief amongst the natives is that the symbols grew there with the rocks.

ALASKA PACKERS ASSOCIATION
SAN FRANCISCO, CALIFORNIA,

A REVIVAL OF THE ANCIENT HOPI POTTERY ART

WHEN the expedition of Major J. W. Powell visited the Hopi in 1872, the East Mesa and Oraibi Indians alone of the seven pueblos made pottery, and that of an inferior quality. Scattered over the soil in the neighborhood of the pueblos, and especially on the sites of the ancient towns, were fragments of vessels of fine ware and excellent decoration which pointed to a golden age of pottery making. A few entire examples of the old ware were brought out by the early exploration, but the full beauty and interest of the ancient pottery became known through the exploration of Dr. J. Walter Fewkes in the ruins of Sikyatki and Awatobi, the former having a dim tradition and the latter terminating its career abruptly about 1700. In the light of the ceramics coming from these villages the decay of the Hopi potter's art was seen to be almost complete. Apparently the perpetuation of Hopi pottery was due to the Tewa from the Rio Grande who settled in Tusayan about 1700 and founded the village of Hano on the East Mesa. They became in fact during the last half century practically the only potters in Tusayan and their art which they had brought originally from the Rio Grande was slightly modified, the decoration being most affected. It is to the credit of an Indian woman, a native of Hano named Nampeo, that the ancient potters' art of the Hopi has been revived. The manner of the happening is interesting. Nampeo's husband Lesu, a Hopi, worked for Dr. Fewkes on the excavations of Sikyatki, and Nampeo often visited the scene of his labors. She became very much interested in the beautiful ware which Dr. Fewkes was recovering from the debris of Sikyatki, and being of an inquiring mind sought the source of the fine clay which the potters of that pueblo converted into the buff and ivory vessels by their handicraft. Nampeo experimented with the Sikyatki clays and found what she considered to be the ancient clay; she matched the pigments used in decoration, and finally copied the designs on paper with a lead pencil. In 1896 the writer saw her in Dr. Fewkes' camp copying the designs on pottery from the middle mesa, and that year secured for the National Museum examples of her first productions. These specimens were full of promise. The ware is of good quality, unslipped like the ancient ware, and the decoration is a rather close copy of the designs secured from individual specimens of ancient work. Gradually Nampeo attained greater freedom in design, and for some years decorated her ware in a style which may be termed transitional. At present she has mastered the vocabulary and alphabet of the ancient designs and applies them with the skill of the potters of long ago. Nampeo is progressive, and as long as she lives her taste and skill will grow. Fortunately her pottery was in demand from the outset. and during the score of years of its production she has through it made a living, and achieved distinction, having become the best known of the Hopi. It is gratifying also to know that Nampeo's example and success have induced a number of Hopi women to take a share in her pottery revival, much to the economic uplift of these Indians. The Hopi are excellent designers, and as they have kept their art free from extraneous influences an interesting development may be predicted. The products of Nampeo's school are worthy of wider notice than they secure from summer visitors to the Hopi. The pottery has attained the quality of form. surface, fire changes, and decoration of the ancient ware which give it artistic standing.

An excellent bust of Nampeo has been made by the sculptor Emry Kopta, who has pursued his art among the Hopi for several years. In burning his figures of the native clay Mr. Kopta has had the advice of Nampeo. Likewise Nampeo has observed the making of plaster molds and slip casting by the sculptor. It is hardly probable that she will utilize these steps in advance. Her fame will rest on her contribution to the revival of an ancient American art, which revival is remarkable in that it is accomplished by the Indians themselves without outside influence.

WALTER HOUGH

DR. ROBERT H. LOWIE, Associate Curator of Anthropology of the American Museum of Natural History, has received a temporary appointment as Assistant Professor in the Department of Anthropology in the University of California for the college year 1917–18. He has been given a year's leave of absence by the American Museum of Natural History. Dr. Lowie is to fill the vacancy caused by the participation of Professor T. T. Waterman in the war. In exchange Professor A. L. Kroeber will join the staff of the American Museum of Natural History during the first half of the year 1918.

THE WORK AT PECOS which Dr. A. V. Kidder is conducting for the Department of Archaeology of Phillips Academy is not to be continued this year.

A JOINT expedition of the Bureau of American Ethnology and the Museum of the American Indian, Heye Foundation, is conducting archaeological excavations at the historic pueblo of Hawikuh, one of the famed "Seven Cities of Cibola," fifteen miles southwest of Zuñi, New Mexico. The investigation is under the immediate direction of Mr. F. W. Hodge of the Bureau of American Ethnology, assisted by Mr. Alanson B. Skinner of the Museum of the American Indian. Active operations were commenced the first of June and important additions to our knowledge of early Zuñi culture have already been accumulated. A fuller report on the results of the research will be presented later in these pages.

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NEW SERIES

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NOTES ON THE POTTERY OF PECOS

BY M. A. AND A. V. KIDDER

THE pueblo of Pecos in San Miguel county, New Mexico, was abandoned in 1838; after a presumably continuous occupation from an early period in prehistoric times. Its rubbish heaps, therefore, contain the accumulations of several centuries, and some of them are so stratified that by a careful study of their contents one is enabled to trace the development of the arts and industries of the community from beginning to end. Of the various objects found in these stratified heaps, pottery fragments are by far the commonest and most easily classifiable, and it is with them that the present paper is concerned.

That there have been changes in the pottery of the Rio Grande pueblos in prehistoric and historic times, has been recognized by archaeologists for a number of years, but no sure evidence as to the sequential order of the several recognizable styles was forthcoming until Mr. Nelson found at San Cristobal a stratified rubbish heap containing wares of three of the principal earlier types. Similar material from other sites enabled him to place chronologically two later types that brought the sequence unbroken down to a comparatively recent date (*ca.* 1700).[1] These investigations served, in the words of Dr. Kroeber, to convert "the archaeological problem of the Southwest from an essentially exploratory and descriptive one, with interpretation based chiefly on Spanish documentary and native legendary sources, into a self-contained historic one." Mr. Nelson's discovery, and his discriminating analysis of his material constitute, therefore, one of the most important contributions that has yet been made to North American archaeology.

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[1] N. C. Nelson, "Chronology of the Tano Ruins, New Mexico," *American Anthropologist* (N.S.), vol. 18, pp. 159–180. Here referred to as "*N.*"

As Pecos was inhabited for a longer time than any other historic Southwestern site available for excavation;[1] and as its compact form, imposed by its position on a small mesa, tended to confine its rubbish heaps to a small area, an almost complete ceramic series can there be recovered in exact order. Thus we are able to cross-reference and, in some respects, to supplement the data secured by Mr. Nelson. We publish our results at this time, some years before the final report on the complete investigation can be brought out, and in their present fragmentary form, in order that they may become available for the constantly increasing number of archaeologists working in the Southwest.

The paper is in three parts: first, a classification of the wares found at Pecos, with condensed descriptions of the different types; second, the presentation and analysis of the stratigraphical evidence which has served to place them in their proper chronological order; third, a discussion of the sequence of the types.

CLASSIFICATION AND DESCRIPTION

In making this study we have used only sherds, the complete vessels found in the excavations not yet having been restored; hence we are unable to give any data on the shapes of whole pieces, or to include more than the most general notes on decoration. As it is difficult to describe in words such subtle qualities as the differences between nearly related groups of pottery, we have picked out a short series of sherds illustrating all the types. This series will be sent (on application to the Department of Archaeology, Phillips Academy, Andover, Mass.) to any one who cares to use it, either for a clearer understanding of the present paper, or for comparison with collections from other parts of the Southwest.

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[1] Acoma is the only pueblo which has not changed its location at least once since 1540, and as to the length of its prehistoric occupation we are ignorant; it is possible, therefore, that Pecos was absolutely the longest inhabited village in Arizona or New Mexico.

The classification follows:

- I. DULL-PAINT WARE.
  - 1. Black-on-white.
  - 2. Biscuit.
    - *A*. Rough exterior.
    - *B*. Smooth exterior.
  - 3. Modern.
- II. GLAZE-PAINT WARE.
  - 1. Two-color.
    - *A*. Black-on-red.
    - *B*. Black-on-yellow.
  - 2. Transitional.
  - 3. Early three-color.
  - 4. Pajaritan three-color.
  - 5. Late three-color.
  - 6. Degenerate.
- III. UNDECORATED SMOOTH WARE.
  - 1. Polished black.
  - 2. Plain red.
- IV. BLACKWARE.
  - 1. Corrugated.
  - 2. Strong blind-corrugated.
  - 3. Medium blind-corrugated.
  - 4. Faint blind-corrugated.
  - 5. Featureless.
  - 6. Striated.

I. DULL-PAINT WARES

1. *Black-on-white*

Rim-shape.—Bowls; sides rise rather straight with no thickening at rim. Lip usually flat, but with rounded corners (pl. 1, fig. 1). Rounded and thin rims (figs. 2 and 3) present but uncommon. About 10 per cent. of bowl-sherds show outcurved rims with rounded lips (fig. 4). Ollas; insufficient data.

Surface-finish.—Slip-color ranges from light to dark gray, very rarely pure white; it is applied to the interior of bowls in a relatively heavy coat; to the exterior usually as a thin wash, occasionally as a heavy coat, rarely omitted altogether. Slip often crackled, particularly when it is applied heavily on the exterior.

Finish of interior even but never glossy; of exterior much rougher. No specimen with corrugated or basket-marked exterior observed.

Ornamentation.—Pigment dull black, usually of a slaty cast. Zones: interiors of bowls, exteriors of ollas; exterior decoration of bowls exceedingly rare. Straight bowl-rims usually plain, occasionally dotted; outcurved rims bear ticks or, more commonly, a single heavy zigzag line. The main ornament appears to have been in the form of a broad band encircling the interior of the bowl and leaving only a small blank space in the bottom. Bands framed above and below by single heavy lines ("*N*," pl. VII, fig. 7); less commonly by one heavy line with a series of lighter ones between it and the band ("*N*," pl. VII, fig. 8). Line-break in framers rare. "All-over" decorations apparently fairly common, particularly on small bowls. Design preponderatingly geometric and rectilinear, life and curvilinear forms practically absent. Elements of design most commonly observed: coarse hatching and cross-hatching; plain and dotted checkerboard; series of plain triangular figures; dotted lines and edges; large stepped figures in opposed pairs. Brushwork normally crude and uncertain, lines coarse.

Paste Composition.—Paste fine, very hard and homogeneous; color ranges from pure white (rare) through light gray to dark gray (rare). Many sherds are of even color from surface to surface, but in the majority the center is darker than the edges. Gross tempering (sand or pounded rock) apparently absent; most sherds show a sparse sprinkling of light and dark flecks (nature undetermined). Average thickness of ware for bowls slightly over $\frac{3}{16}$ inch; for ollas about $\frac{3}{16}$ inch.

2. *Biscuit-ware*

Biscuit-ware is a soft, thick, and porous pottery with light colored slip and dull black paint. It is confined pretty strictly to the Rio Grande drainage, where it is most common in the northern districts: Taos, Abiquiu, the Chama, the Oso basin, etc. In the Pajarito country its abundance relative to other wares varies at different sites;[1] at Pecos and in the Galisteo basin it is less frequent than in the Pajarito and Keres regions ("*N*," p. 169); in the Manzanos and along the Rio Grande in southern New Mexico it is apparently rare. Its chronological position and morphological relationships will be discussed below. The division of the ware into two groups (*A* and *B*) is based on technological and decorative features which are believed to represent more than mere variation within a single group. Type *A* comprises all pieces which have

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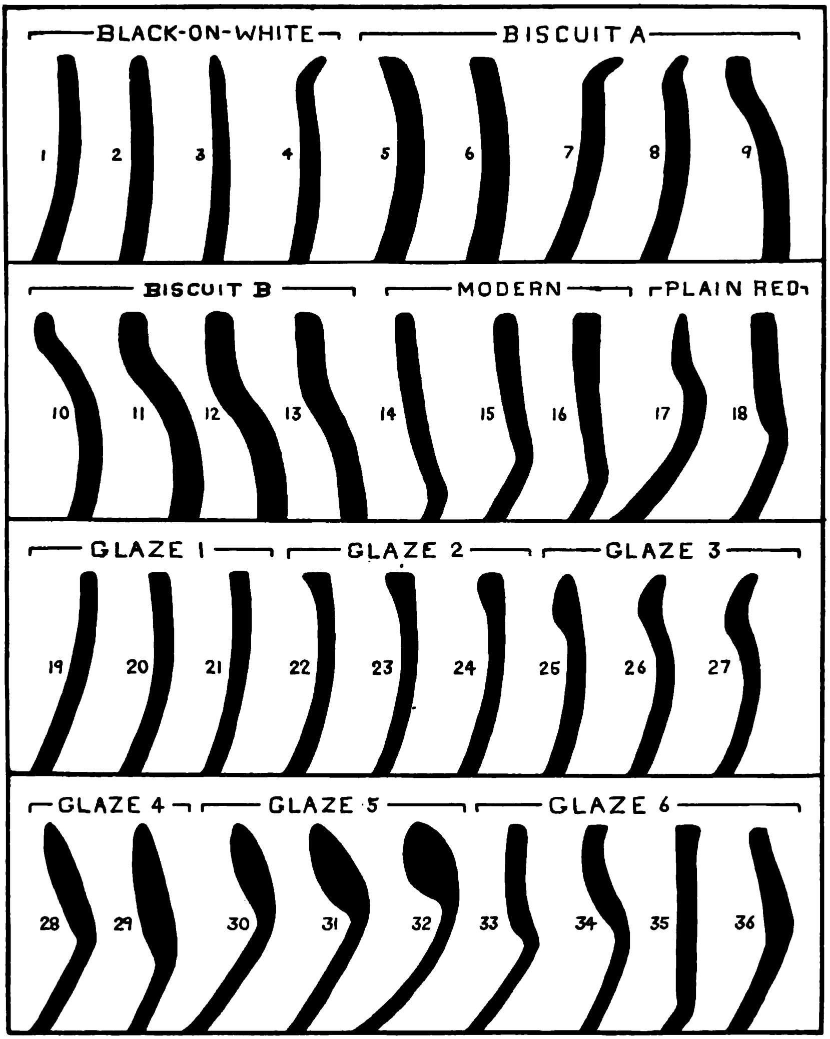
[1] See Kidder, "Pottery of the Pajarito Plateau and of some Adjacent Regions in New Mexico," *Memoirs American Anthropological Association*, vol. II, part 6, p. 459. Here referred to as "*K*."



BOWL-RIM SECTIONS

unslipped rough exteriors; Type *B* those with slipped and decorated exteriors. Further differences between the two will be brought out in the following descriptions:

Biscuit A

This particular variety has hitherto been reported from only one site, the Agua Fria "Schoolhouse" ruin ("*K*," p. 449); its presence in the Galisteo basin is to be inferred from Nelson's description of biscuit-ware with "decoration on one side" (p. 169).

Rim-shape.—Bowls (ollas not identified): there are three types; the plain (pl. I, figs. 5, 6), the outcurved (figs. 7, 8), and the standing (fig. 9); the outcurved is much the most common. In all three the bowl-sides are more strongly curved than in black-on-white bowls (*cf.* figs. 1 to 4). Plain type: the lip is essentially flat and horizontal; the exterior edge of the flat top is rounded, the interior sharp (fig. 5). Outcurved type: the lip is somewhat thinner than the side of the vessel and has a rounded termination; the width of the flare, its angle, and the extent of its thinning, vary considerably (figs. 7, 8). Standing type: the rim proper is vertical; the lip is flat-topped with rounded edges.

Surface-finish.—Slip a very even, dull light-gray (very seldom if ever yellowish); well smoothed but seldom "slick" and never polished; rarely crackled; is applied to interior only. The unslipped exterior has a very characteristic texture, showing the close-set and irregularly horizontal striations of a scraping tool; the contours, however, are even and true, much more so than the rather irregular exteriors of black-on-white bowls; color dark gray.

Ornamentation.—On interior only. Pigment a dull, deep black, lacking the bluish cast of black-on-white. The lips of some of the straight-rimmed and most of the standing pieces are dotted; the inner surfaces of the standing examples and the flaring part of the outcurved ones are decorated with: a single zigzag line; or a series of dextrally oblique dashes; or with small, elongated rectangles containing one, two, or three parallel horizontal lines. The main design is apparently almost always of band type, leaving a rather large clear space in the bowl-bottom. The bands are usually framed above and below by single very heavy lines, less commonly by narrower single lines with interior sets of minor framers. Line-break not noted. Elements of design (such as key-figure, hatching, cross-hatching, and checkerboard) seem to be closer to the black-on-white system than to biscuit *B*, though a few motifs characteristic of the latter (awanyu and standard triangle—see "*K*," pl. XVIII, and fig. 13) seem to be present in an undeveloped form. Brushwork averages coarser and more irregular than in black-on-white.

Paste Composition.—Less compact, softer and more porous than black-on-white; fractures more irregularly; has a characteristic "woodeny" feel, and lack of ring when struck. Where the color is not uniform from surface to surface the pieces always become progressively darker toward the exterior. No gross tempering at all; a few white flecks through the paste. Average thickness of bowl $\frac{5}{16}$ inch.

Biscuit B

This ware is characterized by the presence of slip and decoration on both interior and exterior of bowls, and by high, square-lipped rims; also by variability of slip-color, which ranges from almost white to almost yellow. Nelson (p. 169) divides the ware into two classes: white and yellow, but we believe these to be merely variants (due to the presence of oxides in the paste, and to differences in firing) from a light gray normal. Biscuit *B* from the Pajarito, which seems much like that of Pecos, is described at some length in "*K*" *passim.*

Rim-shape.—Bowls (ollas not yet distinguishable from biscuit *A*, so are here disregarded): standing rim practically the only type, flaring or unmodified examples being rare. The upper wall of the bowl curves inward and the rim rises more or less vertically without any thickening. Lip flat and horizontal with rounded corners. The rim cross-sections given (pl. 1, figs. 10, 11, 12, 13) are believed to represent a development from early to late.

Surface-finish.—Slip applied to exteriors as well as interiors; usually dull light gray, some pieces fired (or smoked in use?) to dark gray, others of a yellowish cast. Surface smooth, but seldom "slick" and never polished. Slip rarely crackled.

Ornamentation.—In dull black pigment like that of type *A*. The lips of bowls regularly are ticked at about ½-inch intervals with large dots of paint. Inner surfaces of rims bear zigzags, festooned lines, vertical or oblique dashes. The body designs are generally of band type both interior and exterior; framed above and below by single heavy lines with series of thinner framers between them and the decoration proper. The design system is comparable to that of Pajaritan biscuit (see "*K*" *passim*) the triangle, awanyu, double flag, cut-down rectangle, and hatched triangle, all appear. Zigzag decorations seem rare, nor have we noticed the cross-bird. The line break is uncommon or perhaps absent.

Paste Composition.—Not distinguishable from biscuit *A* in consistency, tempering or thickness; but pieces of yellowish shade are commoner and average brighter yellow.

3. *Modern Painted*

This ware bears so strong a family resemblance to the present-day decorated pottery of certain of the eastern pueblos, *viz.*, Santo Domingo, Cochiti, San Ildefonso and Tesuque, that did stratigraphical evidence not show it to be of late date its modernity could safely be inferred on comparative grounds. The question of the ancestry of modern ware is discussed in the concluding section.

Rim-shape.—Bowls: very characteristic, high (from ½ to 3 inches, average about 1½ inches), and gently curved (see pl. I, figs. 14, 15, 16): lip flat with rounded edges. Small, shallow bowls with plain rims present but rare. Ollas: little data, but apparently always flaring with round lip (*cf.* pl. II, fig. 6).

Surface-finish.—Slip in two colors: red and yellowish-buff.[1] The former is a heavy soft coat, susceptible to wear and to chipping; it is very commonly crackled; usually well finished, often "slick," rarely glossy. The red slip is brighter than the reds of glazed ware, is softer and is brought to a *well-polished* surface. For range of the slips see under ornamentation.

Ornamentation.—In dull black pigment not distinguishable from that of biscuit, red paint used for fillings in a few cases. Zones: on bowls usually only the exterior of the rim, that surface being covered (from outer edge of lip down to the shoulder) with light slip to receive the design; in these cases the light slip is underlaid by the red slip which extends from 1 or 2 inches below the shoulder over the lip to (but seldom beyond) its inner edge. The whole interior of the bowl and its exterior bottom are left in the paste, which is only fairly well smoothed. A few bowls are decorated in the interior (as well as on the exterior) in which cases the light slip also extends over that surface. Ollas: little data as to zones; some apparently decorated in black over light slip from shoulder to lip; others in a narrow band about, or just above, shoulder. In all cases the polished red slip extends from well below the shoulder to well inside the rim, underlying whatever application there may be of the light slip. Design of exterior of bowl rims (insufficient data for ollas and bowl interiors) consists of a band, always framed below, and usually also above, by a single narrow line. Panelling and the repetition of a single element are characteristic. Terraced figures, cloud-festoons, birds (?) and what appear to be vegetal forms are common. Lines are thin and brushwork is somewhat more confident than in biscuit *B*.

Paste Composition.—Harder and less porous than biscuit but, like biscuit, light and "woodeny." Color occasionally dull orange throughout but most commonly very dark gray (almost black) in a central streak which changes to yellow and finally to orange toward the two surfaces. The yellow-orange surface streaks vary in thickness according, apparently, to the intensity of the firing. In most sherds they occur under both surfaces, but are usually stronger under the outer; in some cases they are present only below the outer one. Tempering of sand or broken rock so rare as to seem accidental where present; most pieces show a few small, pure-white flecks which are more striking in this ware than are the corresponding light flecks in the biscuit and black-on-white because of the darker shades of the paste. Thickness of ware for bowls just less than $\frac{1}{4}$ inch; for ollas about $\frac{3}{16}$ inch.

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[1] Occasionally, though very rarely, pinkish as in the Tano specimens; "*N*," p. 177.

II. GLAZE-PAINT WARES[1]

1. *Two-color*

This class is identical with Agua Fria "Schoolhouse" pottery[2] and with Mr. Nelson's chronological Type II.[3] It is characterized by decoration in thin black glaze applied over red or light colored slips. The red and the light varieties are considered as color phases of a single type because they are found together, are practically identical in vessel-forms, and are very similar in decoration. Mr. Nelson has shown that the red began to be made somewhat earlier than did the light-colored, and our own stratigraphical studies tend to confirm this.

Rim-shape.—Bowls: the general trend of the sides indicates a somewhat more open vessel than in black-on-white or biscuit. The rim is invariably unthickened and plain, the lip tending to squareness, particularly in the yellow examples (pl. 1, figs. 19, 20). The modeling of pieces is very precise and even, the least asymmetry in thickness or in line being unusual. Ollas: rim always straight, with no thickening or flare at the lip (the outline given in plate 2, fig. 1, is characteristic).

Surface Finish.—Slip; a relatively heavy coat; either a deep rich red; or a light color, varying from light gray to clear lemon-yellow. It seems to cover all surfaces of bowls and whole exterior of ollas. Excellently smoothed, but not polished; no crackling.

Ornamentation.—Glaze-paint. On light colored pieces the glaze is usually thin and "varnishy"; holds well to its lines with little or no running. The most brilliant, lustrous, and blackest glaze occurs on light gray or whitish pieces; on yellower surfaces it is browner, less lustrous and more "varnishy." The glaze, when applied over the red slip, is black, except where thin (then brownish); when extra thick it tends to draw together in little flat "beads," which are very characteristic. Zones of decoration: bowls, interior only (a few oblique dashes or crosses are the sole form of exterior ornament); ollas, upper body from shoulder to base of rim, latter undecorated. Arrangement: a single broad, horizontal band for ollas; the same for all (?) red and some light bowls; many light bowls have designs in the form of long, thin, intensively ornamented triangles which are set horizontally just below the rim (for a good illustration see "*N*," pl. VII, fig. 5). Elements: geometric and strictly rectilinear; opposed key figures,

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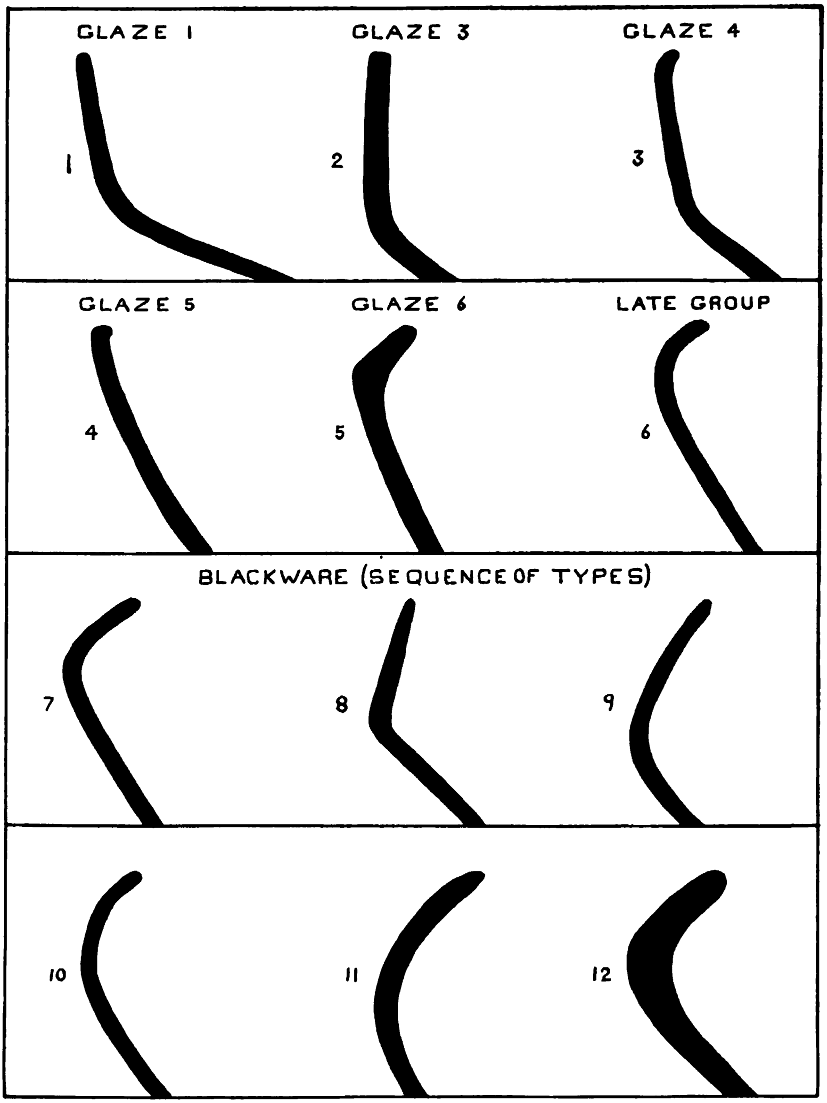
[1] For discussions of the distribution of these wares see "*N*," p. 160; "*K*," pp. 453 ff.

[2] "*K*," p. 447.

[3] "*N*," p. 172.



OLLA-RIM SECTIONS

hatching, stepped lines, dotted checkerboard, series of small right triangles depending from lines (*cf.* Nelson's description, p. 173). Brushwork even and confident; lines narrow and true.

Paste Composition.—Hard and ringing when struck. Color very variable: gray, yellowish-gray, yellow or even orange; some pieces have a gray central streak with yellow-orange layers under one or both surfaces. No gross tempering, fine white flecks sown thicker than in black-on-white or biscuit. Thickness of ware: bowls $\frac{3}{16}$ inch on the average; ollas a trifle less.

2. *Transitional*

In our preliminary classification, this type was set up as a sort of catch-all for pieces that did not accord well with the standards which were being used for the separation of Types 1 and 3. When these pieces were examined together it was seen that, with some exceptions, they formed not merely a collection of unclassifiable examples or chance variants from the normal of the two types, but a true class technically intermediate between them. In many ways the ware is very similar to "Frijolito" pottery ("*K*," p. 451).

Rim-shape.—Bowls (ollas not as yet surely recognized): even sides, that is with only one curve for each surface, but with more or less strongly thickened rims. The thickening is always on the inside and is always greatest at the edge of the lip; hence the lip itself is flat; it is also horizontal (see pl. 1, figs. 22, 23, 24).

Surface-finish.—Slip usually yellowish or pinkish-gray; red slip rare. Is of poorer quality than Type 1, tending to rub and wear off, showing the reddish paste. The light slip covers whole interior of bowls, and exterior except for the bottom, which is red. Surfaces less smooth than in Type 1.

Ornamentation.—Glaze thin, black, and not often "runny." Is seldom highly lustrous, and has often failed to "take" at all, being merely dull black. Zones: the interior *and exterior* of bowls. Interior decoration always apparently of band type, framed above and below by single broad lines; elements somewhat simplified in comparison to Type 1 (see illustrations of pottery of a very similar nature in "*K*," pl. XXVIII, fig. 4). Exterior decoration in the form of crosses, oblique marks, or red-filled double-ended key figures.

Paste Composition.—Not noticeably different from that of Type 1, except in greater thickness of the ware.

Summary.—Type 2 resembles Type 1 and differs from 3 in: shape (even sides); yellower average color of slip; quality of glaze. It differs from 1 and resembles 3 in: outside decoration; red-filled designs. It resembles neither in: thickened but flat-topped bowl rims. Its transitional nature is most clearly seen in the interior decoration, which lies midway between 1 and 3; in the simplification of design and in the coarsening of the brushwork.

3. *Early Three-color*

Although many pieces of the preceding type show three-color decoration, the practice of using red fillings between glaze lines over a light slip does not become firmly established until we reach the present type. This ware, with our Types 4 and 5, makes up Mr. Nelson's Type III ("Three-color glazed ware;" "*N*," p. 173); those pieces of our Type 2 which have three-color decoration he places in his Type III; those without three colors he assigns to his Type II (see fourth bowl-rim from the left under Type II in his fig. 21).[1]

Rim-shape.—Bowls: almost infinitely variable in minor details of curve and proportion, but always thickened *on the interior a little below the lip*, and almost always having on the exterior opposite the thickening a more or less pronounced outcurve (pl. I, figs. 26, 27). A very few pieces have evenly curved exteriors (fig. 25). Ollas: the rim rises almost vertically (pl. II, fig. 2) or with a very slight inward trend; there is seldom any thickening or modification of the lip.

Surface-finish.—Slip of yellowish or pinkish-gray but of more pallid shades than in Type 2. Many pieces are of a bluish or slaty gray-white; this is evidently due to overfiring and is most commonly seen on exteriors of bowls near their rims; fire clouds common. Slip does not crackle, but wears and chips rather badly. Slip covers whole of bowls except exterior bottom and whole exterior of ollas from the inside of the spring of the neck to just below shoulder; the bottom being red. Finish markedly inferior to Type 1, almost never "slick."

Ornamentation.—Glaze averages brown in color (green and black variants) is heavy and shows some tendency to run. It is usually highly lustrous, but the luster is uneven, being finest where the glaze is heaviest and dying down to a "varnishy" appearance where it is thin. Little blow-holes (due to gas bubbles probably) appear in some heavily glazed lines and are yellow or greeny brown. Lines of decoration heavy and no fine-line work occurs. Ollas have red-filled ornaments about the neck; plain ornament on body from base of neck to shoulder. Bowls: red-filled exterior decoration, plain ornament in interior; the inner surface of the flare of the rim is very often marked with a single heavy zigzag line, less commonly with oblique dashes or parts of zigzags. The interior decorations of bowls and the body designs of ollas are usually horizontal bands, framed above and below with pairs of heavy lines (not red-filled). The bands are panelled by upright lines, and the resultant spaces filled with oblique elements of simple nature.

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[1] Reliance upon a single classificational criterion has led Mr. Nelson to misplace one of the ollas in his chronological series (pl. VII). The piece in question is two-color, and he has accordingly grouped it with Type II; its rim-shape and decoration, however, show that it really belongs in his Type III and our Type 4. It is, as a matter of fact, a later piece even than his Type III example.

The "Pajaritan bird" is absent. The exteriors of bowls are always decorated with some form of red-filled key or bird figure.

Paste Composition.—The body of the ware is compact and hard. There is a central gray streak becoming brown or reddish-yellow toward the two surfaces. Most sherds show considerable gross tempering in the form of crushed basaltic (?) rock. Thickness of bowl-walls averages $\frac{3}{16}$ inch, ollas a trifle less.

4. *Pajaritan Three-color*

As its name implies, this ware is similar to the commonest glazed pottery of the Pajarito plateau (that found at Tyuonyi, Tschirege, Otowi, Tsankawi, Puyé).[1] At Pecos it is not very abundant, and its exact place in the ceramic series is not yet clear. It evidently occurs in the Galisteo basin, a typical olla being figured by Mr. Nelson (pl. VII, fig. 6).

Rim-shape.—Bowls: very high, thickened, and with rounded lip; exterior of rim generally flattish (pl. I, figs. 28, 29), rarely a trifle concave; inner side always has a long, even, convex curve. Ollas: rim straight but inclines a little inwards, lip usually recurved outwards (pl. II, fig. 3).

Surface-finish.—Slip in varying shades of light red (very rarely pinkish white), covers all surfaces of bowls, and exteriors of ollas from bottom of interior of rim to well below shoulder, perhaps also bottom. Is carefully smoothed, but seldom "slick" and never glossy.

Ornamentation.—Pigment is black to brown glaze with a high average lustre and fair tractability (not much running). Fillings of many designs are in a red paint much darker in color than the slip. Zones of decoration: interiors of bowls, from base of standing rim downwards; exteriors, from just below rim-edge to about one inch below the under-curve of the bowl-body. Ollas: the exterior of the neck and the upper-body, the two usually being treated as distinct zones. Interior of bowl-rim generally plain, sometimes bears a zigzag line as in Type 3. Bowl-design: interiors have bands or, less commonly, "all-over" patterns. The former are framed with two lines above and two below, each set red-filled. The figures in the bands and in the "all-over" decorations are also often red-filled. Bands are regularly panelled by sets of vertical bars, the panels bearing alternating elements, usually birds and oblique figures (see "*K*," pls. XXII–XXIV). Exterior decoration generally in the form of a double line filled with red drawn about bowl below standing rim; from it rise at intervals, birds or other elements, often red-filled. Independent repeated units (*i. e.*, not attached to a line), such as double-ended keys and three-finger designs ("*K*," pl. XXV and fig. 19) are only fairly common. Olla-design: as a rule the neck decoration follows that of the bowl-exterior; body decoration that of the bowl-interior.

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[1] Described with illustrations in "*K*," pp. 417–447.

Paste Composition.—Not distinguishable from that of Type 3 in color, but some examples have considerable gross tempering in the form of smooth-edged bits of quartz (water-worn ?). Average thickness of bowl-walls $\frac{3}{16}$ inch, of ollas a very little less.

5. *Late Three-color*

This is by far the commonest single type found at Pecos; it never occurs, as far as we know, in the Pajarito ruins; the heavy bowl-rim illustrated by Mr. Nelson among his Type III examples (third from the left, fig. 21) seems to show that it is present in the Galisteo basin.

Rim-shape.—Bowls: low, very thick, and strongly incurved; interior of rim more or less convex, according to amount of thickening; lip rounded (see figs. 30, 31, 32). Ollas: the rim continues the curve of the vessel-body without abrupt change of angle, the lip is flat-topped, with a slight flare (fig. 4, pl. 2). Olla-bowls: these are bowls with such restricted apertures as to approach the olla form (see "*K*," pl. XVII, fig. 3; and "*N*," fig. 21, second from the right, top row). At Pecos this form seems to be present only in the Type 5 glazes. In shape of rim the examples are closely similar to the one figured by Mr. Nelson.

Surface-finish.—Slip-color dirty white (commonest), through cream to yellow; rarely red. Slip covers the whole interior of bowls, and exterior nearly to bottom (which is either unslipped, or painted with a thin red wash); ollas, light slip from inside of lip to just below shoulder; bottom, and inside of rim, red. The light slip is a heavy coat, the thickness of writing paper; it is soft, and wears badly; crackles only in heavily overfired pieces. Finish averages poor.

Ornamentation.—The pigment is a very heavy glaze, which varies from dull and "crusty," to a fine lustrous brown with iridescent lights; where it runs out thin it is light-green to canary-yellow. The drips and runs of the glaze are always toward the rim. Red fillings are in a thick, soft pigment which frequently flakes off. Decorative zones: bowls; interior, and upper part of exterior; ollas, neck and upper body. Bowl-design on exterior consists of a red-filled double line encircling the vessel a little below the rim, with crudely drawn red-filled figures, usually birds, perched upon it; a variant of the three-finger decoration ("*K*," fig. 19), made to imitate the human face, is common; double-ended keys are apparently absent. The inner decoration of bowls is seldom more than a pair of red-filled lines running about the pot just below the rim-swelling. Ollas: data insufficient, consists apparently of simple red-filled bands and very crude bird figures. All lines are heavy and carelessly drawn, the potters evidently having realized that the glaze would run in firing and spoil any attempt at fine work.

Paste Composition.—Color: those pieces which have the clearest yellow slip and the most lustrous glaze, are light gray clear through; those with poor glaze and slip are browny-gray in the center and reddish toward the edges. The paste is granular and "sandy," rather thickly tempered with small, dull-white masses (ground-up sherds?); there are sometimes a few grains of quartz. Average thickness for both bowl- and olla-walls, a little less than ¼ inch.

6. *Degenerate Glaze*

This type corresponds to Mr. Nelson's Type IV. As he points out (p. 175), it is of historic date, and is characterized by diversity of form and by execrable decoration. The ware has so far proved to be much less common at Pecos than at San Cristobal and San Marcos, and to be represented by a smaller range of forms, nothing but bowls and ollas having been found. Our excavations, however, have not yet penetrated to those parts of the site which contain the largest amounts of degenerate glaze.

Rim-shape.—Bowls: extremely varied, but in general there is little thickening (as compared to Type 5), the rims are high, usually nearly vertical, and have a tendency toward reverse curves. A series of four rim-sections is given (pl. I, figs. 33–36); these represent the commonest forms, but no two bowl-rims are ever quite identical. Ollas: the neck continues the slope of the body without abrupt change of angle, the rim is thickened, and the lip is strongly outcurved (pl. II, fig. 5).

Surface-finish.—Slip usually light colored, less commonly red. The light slip is very variable in shade: gray, greeny-gray, pinkish, cream, yellow; it covers the whole interior of bowls, and the exterior nearly to the bottom, which is painted red. Ollas: light-slipped from just within the rim to a little below the point of greatest diameter, bottoms red. Finish generally poor, irregular and "scrapy."

Ornamentation.—The glaze is thick, heavy, and extremely "runny." In color it is usually dark green or dark brown, but where thin is often light green or even yellow. Many examples are dull and poor; where the glaze has "taken" well, however, it is beautifully lustrous. All attempt at precise design is abandoned, the decoration consisting of crude zigzags, blobs, dashes, etc., applied to the exteriors and interiors of bowls and the upper parts of ollas (see "*N*," pl. VII, figs. 1 and 2). There are no red fillings.

Paste Composition.—Variable. Color generally dark gray throughout. Some pieces are very heavily tempered with bits of crushed quartz, others have no tempering beyond a few light-colored flecks. Vessel-walls average thinner than in Type 5.

III. UNDECORATED SMOOTH WARE

1. *Polished Black*

Pottery with high-polished, lustrous black surfaces is found, in the Southwest, on the lower Gila (Arizona), in the Casas Grandes district (Chihuahua), in the Socorro region of New Mexico, and at

Pecos; it is also made today at the pueblo of Santa Clara. The significance of this peculiar and seemingly patchy distribution has yet to be worked out. At Pecos the ware is surely late and is probably all of historic date.

Rim-shape.—Ollas: (bowls very rare) the rim flares out strongly and has a rounded lip (pl. 2, fig. 6).

Surface-finish.—Slip deep black in most cases, occasionally slate-gray; covers whole exterior and runs down about an inch inside the rim. The characteristic polishing of the surface extends from within the rim to well below the shoulder (possibly also over under-body and bottom); the average pieces are only fairly well polished, the best have a perfectly even texture and high sheen.

Paste Composition.—Color deep chocolate brown to almost jet black; thickly tempered with very fine particles of what seems to be pounded quartz; this gives the paste a granular or sandy appearance. The ware is a little harder than biscuit, but like biscuit has a "woodeny" feel. Average thickness $\frac{3}{16}$ inch.

2. *Plain Red*

This ware may be distinguished, even in small sherds, from undecorated red fragments of other classes (such as the bottoms of Types 3, 5, and 6 glaze ollas, bits of Type 1 red bowls, etc.) by its highly polished surfaces and by the color and "feel" of its paste.

Rim-shape.—Bowls: much like modern (see pl. 1, fig. 18) for larger specimens; the smaller bowls (fig. 17) have lower rims with sharp or rounded lips. Ollas do not differ in rim-shape from polished black (*cf.* fig. 6, pl. II).

Surface-finish.—Slip bright red; covers whole interior of bowls, and often the whole exterior as well, the bottoms of some examples are left unslipped. Ollas: the slip runs from within the rim nearly or quite to the bottom. The finish ranges from indifferent to very highly polished, the finest specimens are always ollas.

Paste Composition.—Apparently exactly the same as modern (*q. v.*).

IV. BLACKWARE

The black cooking pottery, as well as the finer, colored ware underwent changes during the occupation of Pecos. These changes, however, were very much less radical than those in the other classes, and took place so slowly that, while there are quite obvious differences between the earliest and the latest sherds, the intermediate stages can only be made out by placing a number of pieces from each stratigraphical layer side by side and considering them, so to speak, in the mass. By doing this several successive styles may be recognized. There are, however, so many unclassifiable fragments (presumably from the bottoms and lower sides of the ollas) that we have not attempted statistical treatment of the blackware types, but give instead a sort of running account of the development of the ware, beginning with the earliest known style.

Indented Corrugated—This is not found at Pecos proper, but is present at other nearby sites. It is the local representative of the very widespread archaic cooking pottery of the Southwest; pottery made by laying up on itself coil by coil a continuous thin strand of clay, all traces of the coil being obliterated on the interiors of the vessels, but no smoothing at all being done on their exteriors. Thus, in well-made pieces (and the proportion of these is remarkably high) the coil on the exterior is sharp and definite, each turn being clearly distinguishable. Further, the coil was notched or indented at short, regular intervals during its application to the growing vessel, producing a curious, almost textile, appearance.[1] Such is the earliest cooking pottery of which we now have knowledge in the Pecos valley; it does not occur at the main ruin. There, in the lowest levels of the rubbish heaps, we find a coiled ware which lacks the indented marks and is called, accordingly, *plain corrugated*. This is obviously a simplified form of the preceding one; in fact the whole earlier development of blackware at Pecos is a progressive simplification, the visible signs of the structural coil being steadily eliminated. The next stage we have called *blind corrugated;* the vessels of this class still show the horizontal coil-ridges, but the latter have been so smoothed over in finishing that the sharp cracks between them have been obliterated. Three successive subtypes are recognizable: the *strong*, *medium*, and *faint*. In the latter the coil-ridges have been so nearly destroyed that the surface of the ware shows merely a faint rippling, so slight as to be perceptible only when the light falls across it at a very low angle. In *featureless black*, the next type, all trace of corrugation has disappeared, the vessel exteriors having an even though rough texture. The latest blackware so far identified is the *striated;* there is, of course, no sign of the coil, but the surface is scored with series of fine and more or less parallel scratchings, crisscrossing over each other in all directions. These were produced, evidently, by some finishing tool used while the clay was moist. Experiment shows that a corncob with the kernels removed most nearly duplicates these marks.

Blackware may also be classified on the basis of rim-shape; but, as was the case with surface-texture, the principal types merge into each other very slowly. Type *A* (pl. II, fig. 7): sharply flaring, but with a well-rounded curve between body and rim. Type *B* (fig. 8): very straight rim with sharp break in angle from body, less flare than in any other type. Type *C* (fig. 9): rim still straight, but higher, with more flare, and with less abrupt (though still noticeable) break in angle. Type *D* (fig. 10): high rim, evenly curved flare. Type *E* (fig. 11): heavy ware, even curve, wider flare. Type *F* (fig. 12): *thickened* rim, very sharp flare, solid construction.

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[1] For a fuller description of the process, with excellent illustrations see Holmes, "Pottery of the Ancient Pueblos," *Fourth Annual Report Bureau of American Ethnology*.

These six rim-types seem to correspond stratigraphically in a general way to the six surface-texture types (striated with *F*, featureless with *E*, faint blind-corrugated with *D*, etc.). Both series of types, however, are vague at best, so that single black sherds cannot be relied upon as sure evidence for the dating of strata or deposits; but masses of them sufficiently large to enable one to discount minor variations and to judge of their average character, would undoubtedly be of value.

STRATIGRAPHICAL

Having classified the wares, we must turn to stratigraphy for our determination of their chronological order. Material for this study was gathered by means of test sections similar to those made by Mr. Nelson; columns of rubbish running from surface to hardpan were isolated, and from them was taken their entire pottery content, the fragments from the different levels or cuts being kept separate The Pecos tests were not divided into exactly equal cuts, as was done by Mr. Nelson, but were laid out in nearly equal divisions based on sand, ash, or other strata which indicated actual levels of deposition during the formation of the mound. This was necessitated by the fact that the Pecos deposits were for the most part laid down on sloping or irregular surfaces, and cuts made on arbitrarily chosen plane levels would have resulted in the splitting or cross-cutting of strata. This method derogates from the absolute statistical value of the material, as the cuts, not being of exactly equal thickness, are not strictly comparable statistically. The cultural results, for which these tests were taken, are, however, satisfactory; and even the statistical outcome seems fairly significant. Columns of debris have been saved intact in all parts of the heap, so that more exactly subdivided sections can be taken if desired.

Of the four tests here considered, three (whole series nos. X, XI, XII) are from the great rubbish heap on the east slope of the Pecos mesa. The earth-column of test X was 20 feet long, 5 feet wide, and 18 feet high; its eight cuts (numbered as in all the others from top to bottom) averaged 2 feet 3 inches in thickness. Tests XI and XII were each 25 feet long, 5 feet wide, and 12 feet high, XI had seven cuts of about 1 foot 8 inches in thickness; XII, eight cuts of 1 foot 6 inches. These tests were made very large in area in order that we might get a great amount of sherds for the preliminary study, and also to neutralize as far as possible the disturbing effect of graves, from which no part of the eastern heap is entirely free. The fourth test (XIII) was made in a midden below the northwest corner of the main pueblo; it was approximately 3 feet square, by 4 feet high, and had seven 7-inch cuts. This section was gone through with greater care than the other three, even the tiniest sherds being saved.[1]

The material was handled as follows: the sherds from each cut were classified according to ware and to vessel-forms, and the numerical results entered under the headings given in the accompanying tables (nos. I, III, V, VII). Further, the glazed bowl-rim sherds were subjected to closer study (for a purpose which will be made clear below); they were classified, counted, and the results entered in tables II, IV, VI, and VIII. Finally one set of charts was prepared to illustrate graphically the distribution in percentages of the principal wares (*i. e.*, all black, all glaze, all biscuit, etc.); and another set to show in the same way the range of the types of glaze.

Looking at the four ware-charts, we see that each one of them contains two approximately normal frequency curves: (1) for glaze; (2) for biscuit. All show black-on-white very strong at the beginning and declining rapidly as glazed ware rises. Furthermore, each one exhibits the start of three other curves: those for modern painted; plain red; and polished black. The line representing rough black runs more or less irregularly across each chart.

Blackware as a class obviously has no chronological significance. It was evidently a somewhat variable, but nevertheless constantly important factor in the total ceramic output of the pueblo during its whole occupation.[2] As to the meaning of the tendency of the

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[1] In X, XI, and XII no fragment less than ¾-inch across was kept; pieces smaller than this can seldom be accurately classified.

[2] We leave out of consideration the sub-types of blackware; these are without doubt chronologically significant, but our data are not yet sufficiently full to allow us to recognize them with certainty.

TABLE I

Test X

	Number of Sherds								Approximate Percentages							
Cut No.	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Black olla rim[1]	44	28	36	28	26	10	38	4	9	5	6	3	4	2	6	3
Black olla body	237	236	173	269	151	109	164	29	46	46	29	34	25	25	26	22
Polished black[2]	51	19	2						10	4	X[6]					
Late plain red[3]	87	51							17	10						
Modern painted[3]	14	11	3						3	2	X					
Glaze bowl rim	19	32	93	85	82	42	32	1	4	6	15	11	13	10	5	1
Glaze bowl body	28	56	139	169	129	102	95	2	5	11	23	22	21	23	15	2
Olla-bowl[4]	1	4	2						X	1	X					
Glaze olla rim	2	3	7	12	10	5	6		X	1	1	1	2	1	1	
Glaze olla body	20	62	105	152	126	113	53		4	12	17	18	21	26	8	
Biscuit *B* bowl rim		4	6	10	12	6	5			1	1	1	2	1	1	
Biscuit *B* bowl body	1	4	24	37	34	10	4	1	X	1	4	5	6	2	1	1
Biscuit *A* bowl rim			2	5	9	10	16				X	1	1	2	3	
Biscuit *A* bowl body	4		4	22	24	33	61		1		1	3	4	7	10	
Biscuit olla[5]			8	9	3						1	1	X			
B.-on-W. bowl rim	1		6	4	1	1	47	28	X		1	X	X	X	7	21
B.-on-W. bowl body	6	1	4	4	4	6	104	60	1	X	1	X	1	1	16	46
B.-on-W. olla					2		9	6					X		1	4
Totals	515	511	614	806	613	447	634	131	100	100	100	100	100	100	100	100

[1] Only ollas occur.

[2] Only ollas occur; body and rim sherds listed together.

[3] Bowls and ollas; body and rim sherds listed together.

[4] Glazed ware; body sherds of olla-bowls are not certainly distinguishable from those of undecorated bowl body sherds, hence only rims are listed.

[5] We cannot yet distinguish Biscuit *A* ollas from Biscuit *B* ollas.

[6] Present, but less than one half of one per cent.

TABLE II

Occurrence of Glazed Ware Types as Shown by Bowl-rims. Test X.

	Number of Sherds								Percentages							
Cut No.	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Type 6	4	1	1						.7	.1	.2					
Type 5	10	15	15						1.9	2.9	2.4					
Type 4		6	22							1.1	3.5					
Type 3	2	5	44	32	5	2			.3	.9	7.1	3.9	.8	.4		
Type 2	1	3	8	35	57	2			.1	.5	1.3	4.3	9.5	.4		
Type 1 (yellow)	2	2	2	10	14	28	15		.3	.3	.3	1.2	2.2	6.2	2.3	
Type 1 (red)			1	8	6	10	17	1			.2	.9	.9	2.2	2.6	.7
Totals	19	32	93	85	82	42	32	1	3.3	5.8	15.0	10.3	13.4	9.2	4.9	.7
Type I (both phases)	2	2	3	18	20	38	32	1	.3	.3	.5	2.1	3.1	8.4	4.9	.7

TABLE III

Test xi

	Number of Sherds							Approximate Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Black olla rim	59	38	23	16	27	15	4	10	8	6	7	6	4	6
Black olla body	165	116	56	64	92	75	17	29	23	16	30	20	21	27
Polished black	41	15	1					7	3	×				
Late plain red	54	32	1					9	7	×				
Modern painted	21	18	1					4	3	×				
Glaze bowl rim	105	116	92	42	79	61	1	18	23	25	20	17	17	2
Glaze bowl body	68	73	76	50	115	82	2	12	15	21	23	25	23	3
Alla-bowl	7	12	12	4				1	2	4	2			
Glaze olla rim	12	19	19	2	8	9		2	4	5	1	2	3	
Glaze olla body	44	62	83	28	53	50		8	12	23	13	12	14	
Biscuit *B* bowl rim	1		1	5	16	4		×		×	2	3	1	
Biscuit *B* bowl body	1	1		3	46	9		×	×		2	10	2	
Biscuit *A* bowl rim				1	3	13	1				×	1	4	2
Biscuit *A* bowl body					8	27	3					2	8	5
Biscuit olla					6							1		
B.-on-W. bowl rim				1	2	4	7				×	×	1	11
B.-on-W. bowl body				1	6	8	23				×	1	2	37
B.-on-W. olla	1				1	1	4	×				×	×	7
Totals	579	502	365	217	462	358	62	100	100	100	100	100	100	100

TABLE IV

Occurrence of Glazed Ware Types as Shown by Bowl-rims. Test xi

	Number of Sherds							Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Type 6	16	11	1					2.7	2.1	.2				
Type 5	83	101	74	15	3			14.3	20.1	19.1	6.8	.6		
Type 4	2	3	12	17				.3	.6	3.2	7.8			
Type 3	2	1	2	8	58	5		.3	.1	.4	3.6	12.5	1.5	
Type 2			1	1	10	20				.2	.4	2.1	5.5	
Type 1 (yellow)			1		4	21				.2		.8	5.8	
Type 1 (red)	2		1	1	4	15	1	.3		.2	.4	.8	4.1	1.6
Totals	105	116	92	42	79	61	1	17.9	22.9	23.5	19.0	16.8	16.9	1.6
Type I (both phases)	2		2	1	8	36	2	.3		.4	.4	1.6	9.9	1.6

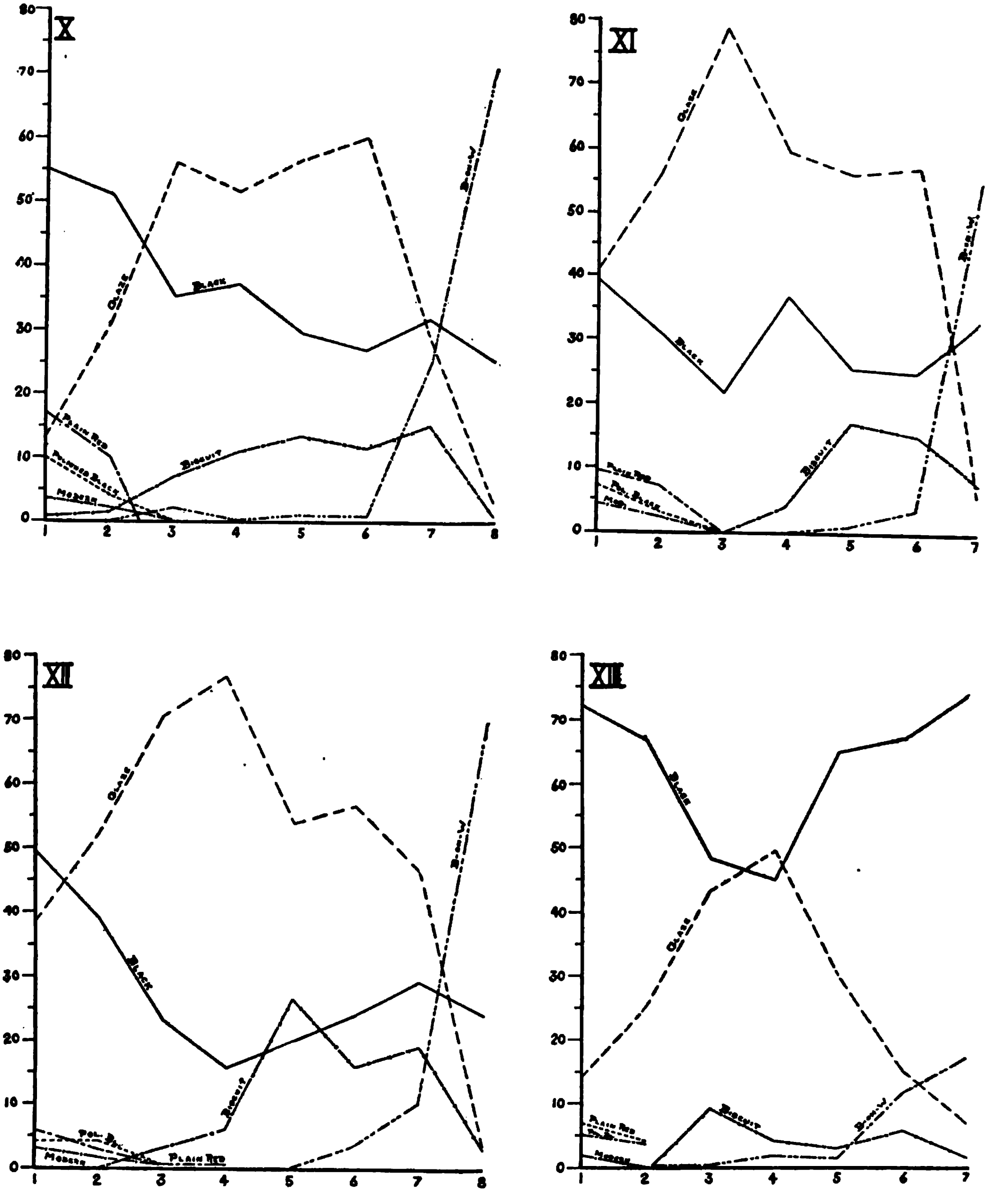

FIG. 54.—Charts illustrating graphically the distribution in percentages of the principal wares.

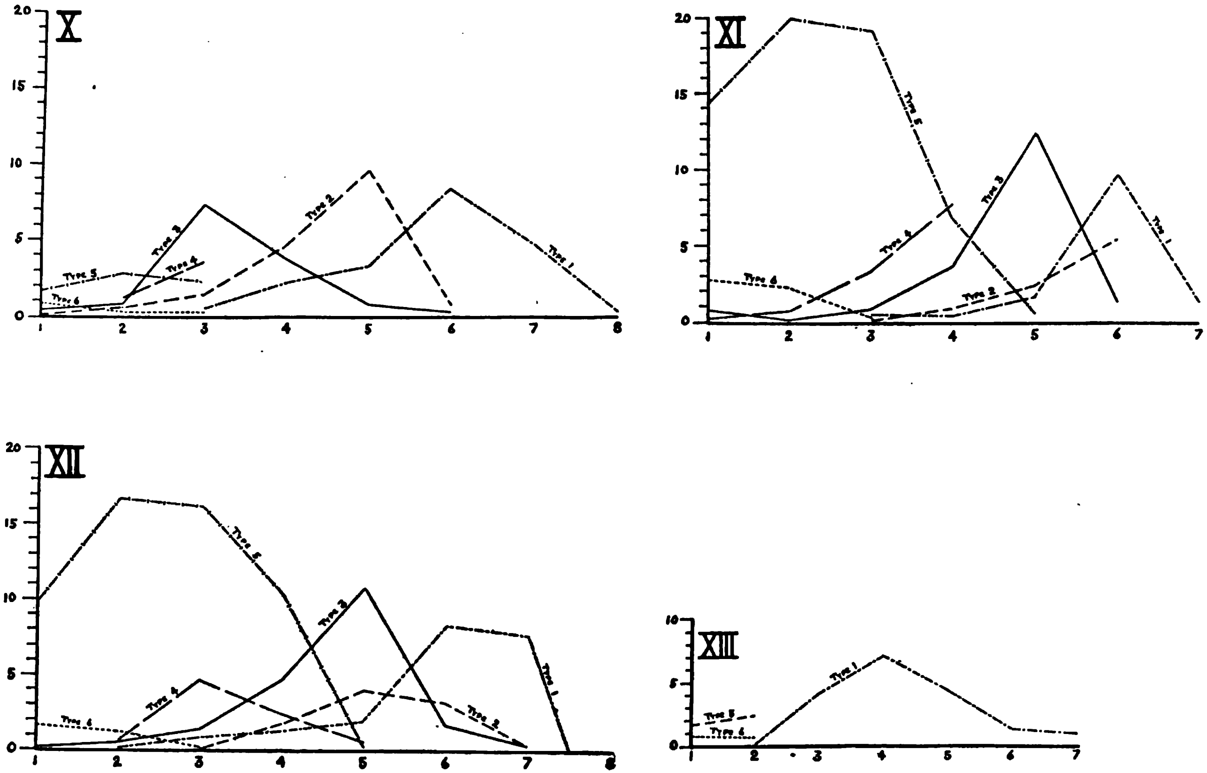

FIG. 55.—Charts illustrating the range of the types of glaze.

TABLE V

TEST XII

	Number of Sherds								Approximate Percentages.							
Cut No.	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Black olla rim	41	132	32	6	20	31	10	6	11	11	7	4	5	5	4	7
Black olla body	143	335	75	20	63	128	66	14	38	28	16	12	15	19	25	17
Polished black	14	54	3						4	4	1					
Late plain red	20	23	4	2					6	3	1	1				
Modern painted	11	25	4						3	2	1					
Glaze bowl rim	43	232	114	34	74	83	23	2	12	19	23	21	18	13	9	2
Glaze bowl body	53	201	104	55	88	131	49	1	14	17	21	33	21	20	18	1
Olla-bowl	2	34	15	3					1	3	3	2				
Glaze olla rim	2	20	20	6	10	17	4		1	2	4	4	2	3	2	
Glaze olla body	35	131	94	29	53	141	35		10	11	20	17	13	21	13	
Biscuit *B* bowl rim		5	2	1	16	8	1			×	×	1	4	1	×	
Biscuit *B* bowl body	1	1	9	1	65	42	4	1	×	×	2	1	16	6	2	1
Biscuit *A* bowl rim				2	3	21	13	2				1	1	3	5	2
Biscuit *A* bowl body	1		5	3	8	36	32		×		1	2	2	6	12	
Biscuit olla		1		1	12	3				×		1	3	×		
B.-on-W. bowl rim		1			2	5	9	18		×			×	1	3	21
B.-on-W. bowl body		1			2	13	15	40		×			×	2	6	48
B.-on-W. olla							3	1							1	1
Totals	366	1,196	481	163	416	659	264	85	100	100	100	100	100	100	100	100

TABLE VI

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST XII

	Number of Sherds								Percentages							
Cut No.	1	2	3	4	5	6	7	8[1]	1	2	3	4	5	6	7	8
Type 6	6	16	1						1.6	1.3	.2					
Type 5	36	201	78	17	1				9.8	16.7	16.2	10.4	.2			
Type 4		8	23	4	2					.6	4.7	2.4	.4			
Type 3	1	6	7	8	46	7	1		.2	.5	1.4	4.8	10.9	1.7	.3	
Type 2			1	3	17	21	1				.2	1.8	4.0	3.1	.3	
Type 1 (yellow)		1	2	2	6	28	7			.1	.4	1.2	1.4	4.2	2.6	
Type 1(red)			2		2	27	14				.4		.4	4.0	5.3	
Totals	43	232	114	34	74	83	23		11.6	19.2	23.5	20.6	17.3	13.0	8.5	
Type I (both phases)		1	4	2	8	55	21			.1	.8	1.2	1.8	8.2	7.9	

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[1] The two glazed bowl rims in cut 8 were too battered for identification.

TABLE VII

TEST XIII

	Number of Sherds							Approximate Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Black olla rim	26	26	2	6	12	22	16	4	3	1	3	3	4	5
Black olla body	415	472	87	90	254	320	234	68	64	47	42	62	63	69
Polished black	43	33						7	4					
Late plain red	30	29						5	4					
Modern painted	11	4						2	×					
Glaze bowl rim	16	26	8	15	19	8	5	3	3	4	7	5	2	1
Glaze bowl body	45	97	36	45	59	41	16	7	13	19	22	14	8	5
Olla-bowl		2							×					
Glaze olla rim		3	3	2	3				×	1	1	1		
Glaze olla body	25	65	35	39	44	26	4	4	9	19	19	10	5	1
Biscuit *B* bowl rim	1		2					×		1				
Biscuit *B* bowl body	1	1	3	3	1			×	×	2	1	1		
Biscuit *A* bowl rim		3	1	1					×	1	1			
Biscuit *A* bowl body		3	10	4	12	29	9		×	5	2	2	6	2
Biscuit olla														
B.-on-W. bowl rim		1		3	2	5	14		×		1	1	1	4
B.-on-W. bowl body	3	1	1	2	6	51	38	×	×	×	1	1	10	11
B.-on-W. olla				1		4	5				×	×	1	2
Totals	616	766	188	211	412	506	341	100	100	100	100	100	100	100

TABLE VIII

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST XIII.

	Number of Sherds							Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Type 6	6	6						.9	.8					
Type 5	10	17						1.7	2.2					
Type 4														
Type 3														
Type 2														
Type 1 (yellow)		2	5	11	8	2			.2	2.6	5.2	1.9	.5	
Type 1 (red)		1	3	4	11	6	5		.1	1.5	1.9	2.6	1.0	1.4
Totals	16	26	8	15	19	8	5	2.6	3.3	4.1	7.1	4.5	1.5	1.4
Type 1 (both phases)		3	8	15	19	8	5		.3	4.1	7.1	4.5	1.5	1.4

blackware curves to dip toward the middle of the charts, and to rise toward their upper ends, we cannot attempt to judge. The total frequency of black relative to other wares seems to depend on the size of the sherds collected. Black vessels break into very many very small fragments, so that if only sherds of 3 inches square were kept, black would fall to almost nothing; whereas if the most minute bits were saved it would rise to well over 65 per cent. An example of this is seen in the plot of test XIII, a section including small fragments.

Black-on-white ware is shown in each chart to drop rapidly from a high initial percentage, its appearance in very small quantities in the middle and upper cuts (as in test x) being due, probably, to churning of the heaps during deposition, or to the dumping of old rubbish from the mesa top. In no case does black-on-white make a respectable showing after the real beginning of the rise of glaze.

Glazed ware is represented by normal frequency curves in all the charts, rising from practically nothing at the bottom, to a high percentage at or somewhat beyond the middle, and declining toward the end little less rapidly than it rose. Although no test is free from glaze in its lowest cuts, nor does any one show the entire extinction of glaze as a ceramic trait, we believe, from evidence other than stratigraphical, that there was a short period at the beginning when the art was not practised, and that no glazed ware was made during the closing years of the occupation of Pecos.

The curves for biscuitware are comparable to those for glaze; both rise together, and both tend to have similar contours. There are, however, these important differences: (1) that biscuit is always a minor or secondary ware in point of frequency; (2) that while glaze runs as far as or even beyond the upper limits of each test, biscuit has a considerably shorter life, its curves becoming negligible in every case while those for glaze are near their highest points.

The remaining three curves: those for polished black, plain red, and modern painted, have their origin at about the point of extinction for biscuit, and are on the rise in the last cut of each test. There is, however, so little overlapping of the biscuit curves and of these three later ones that we do not believe that the wares actually overlapped in time. It seems more probable that polished black, plain red, and modern painted are all parts of a single ceramic group that replaced the glazed wares.[1] This we may call for convenience the late group.

To sum up: it appears that there were three main stages in the ceramic history of Pecos: the black-on-white, the glazed, and the late; that the black-on-white ended soon after the settlement of the mesa, and the late began only shortly before its abandonment. Within the glazed period, then, falls most of the history of Pecos. For this reason a close study of the glazed ware is essential and is greatly facilitated by the fact that the pottery is subdivisible into six easily recognized types.

A cursory examination of the test material, when sorted and laid out cut by cut, shows that three of these glazed ware types, namely nos. 1, 3, and 5, succeed each other in the order given. The chronological positions of the three others (nos. 2, 4, and 6) are strongly indicated: (1) by their occurrence at certain levels in the tests; (2) by technological and decorative features. One would like, however, to have more tangible evidence as to the exact relations of these latter to the more obviously placeable groups (1, 3, 5) than can be had by a mere inspection of the test material. For this purpose we have plotted the distribution of all six types.[2] In the resultant diagrams each of the plotted types tends to form a normal frequency curve, indicating that each one had a natural rise, vogue, and decline. One's general impression is that they are all successive phases of the single glazed pottery trait, and that each one of them developed from its predecessor.

As to the position of Types 1, 3, and 5 there can be, as was remarked above, no doubt. Type 1 is surely the oldest, it being the only one represented in the bottom cut of each of the tests; in test X, indeed, it is the only type in the two bottom cuts, and it stands by itself in the five lowest cuts of test XIII. In test X it is succeeded quite definitely by Type 2, which in turn reaches its maximum and is well along in decline before the real rise of Type 3. In tests XI and XII the evidence for placing Type 2 between 1 and 3 is somewhat less decisive, and were it not for test X and for the marked transitional appearance of the ware itself, we might perhaps have been led to consider it as a contemporary variant of Type 3.

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[1] Here we seem to get a hint that when a style breaks up it is succeeded by two or more new styles (as black-on-white by glazed and biscuit); and that one of these eventually dominates. More research is needed.

[2] As it is impossible to classify accurately undecorated sherds from glazed vessels, and as there is often uncertainty even in the case of decorated fragments which do not show the rim-contour, we have used only rim-sherds in making the type distribution study. What is lost in bulk of material is more than made up for in certainty of identification.

In the case of Types 4 and 5 we are confronted with rather greater difficulty. Type 5, as is shown by the plots of tests XI and XII, is without question a later style than 3; furthermore it has from the technical and decorative points of view the appearance of being a direct outgrowth from it. Type 4, according to the plots, lies within the limits of distribution of Type 5; in tests X and XI it comes in abruptly; nowhere do we get any direct evidence, as was the case with Type 2 in test X, for calling Type 4 transitional or even chronologically intermediate between 3 and 5. The ware itself does not help us, as it fails to fit well into what seems to be the developmental series. One point, however, should be noted: that the curves for Types 2 and 4 are very similar in test XI; and also very similar (though of different form from those in test XI) in test XII. Type 2 is fairly well established as transitional, so that the resemblance between its curves and those of Type 4 may be significant.

Type 6 is shown by the plots to begin at about the time of the maximum frequency of Type 5; it is in the ascendant at the top of each test. The glazed trait as a whole, however, being decidedly on the wane at the top of each test, we may reasonably consider Type 6 to be the last phase of the industry.

Summing up the foregoing, we see that Type 1 is the earliest; Type 2 is without much doubt transitional between it and Type 3, which latter in turn may develop directly into Type 5; the exact morphological position of Type 4 is still uncertain, but chronologically it is either between 3 and 5 or contemporaneous with early 5; Type 6 represents the last stage of glazing.

The value of glazed ware as an archaeological index for Pecos is now obvious. It was made from almost the beginning to shortly before the end, and it may be subdivided into easily recognized types which are chronologically sequent. Glazed ware can therefore be used for assigning relative dates to burials, refuse beds, rooms, kivas, etc.; and also as a criterion for assigning to their proper chronological positions other wares, types of artifacts and the like, which are found in conjunction with one or another of its phases.[1]

Having made out the succession of the three main ware classes, black-on-white, glaze, and late, and having also arranged the principal types of glazed ware in their proper order, we have built up the framework, so to speak, of the ceramic history of Pecos. The greater part of our work, however, still remains to be done; for it is more important to know how and why the observed changes took place than to have merely learned that they did so. The method to be pursued in further research is clear: we must make an intensive study of the wares themselves. For this we must have not only large amounts of material, but also as nearly as possible unmixed material, from each archaeological horizon. The purest representation of any given period is, of course, to be derived from isolated sites inhabited only during that period. For the earlier stages such sites can easily be found, as in early times the people lived in small scattered communities, which often shifted their locations.[2] The one-period site of later times is, however, much rarer, the Pueblos then having been gathered together in large and more or less permanently established towns whose rubbish heaps contain the inevitably somewhat mixed debris of several periods. Isolated heaps must therefore usually take the place of isolated sites; and, though they are difficult to find, careful search at any large ruin will be rewarded by the discovery of pure deposits.[3]

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[1] Black-on-white, biscuit, modern, and even black, are all undoubtedly subdivisible and will eventually also be of use for the above purposes.

[2] *Cf.* "*N*," p. 179.

[3] Most often to be found among the houses, in places where rooms have fallen temporarily into disuse, have been filled with dumped rubbish, and have then been sealed up by the construction of new floors.

For certain transitions, and for periods from which no isolated heaps can be found, we must, however, turn to the large middens, and make stratigraphical tests. To pick out only the best for the illustration of the desired horizons, we must be able to analyze these tests understandingly. To make this point clear, let us consider the ones we have before us.

Test X, according to its glaze-type chart, contains an orderly deposition of Types 1, 2, and 3. Types 4, 5, and 6, on the other hand, are very weakly represented and their curves are far from normal. We conclude, therefore, that at the end of the Type 3 period the heap was practically abandoned as a dump, and that its top cuts accumulated so slowly as to be nearly valueless for close study.[1] We should, then, concentrate on the six bottom cuts for information as to Types 1, 2, and 3. Had we been guided in this case by the total-glaze curve alone, we should have fallen into error, as we would doubtless have believed (because the total-glaze curve falls in this test most nearly to zero) that the upper cuts would give us excellent material for the concluding phases of the ware.

Tests XI and XII are quite different from X, but are similar to each other. The plotted analyses of their glaze types show that their lower parts were built up much more slowly than was the case with X (two cuts from the bottom to the apex of Type 3, as against six cuts in test X); whereas they have five cuts containing the strong curves for Types 4, 5, and 6 (as against two cuts in X with very weak curves). Interpreting this, we see that tests XI and XII came slowly at first, but finished rapidly; they are therefore best for the study of the later glazes. Test XIII is in some ways the most useful of all, though this fact would not be suspected by examining its ware-group chart alone, for there the curve for total-glaze is much like the glaze curves of the other tests. Turning to the glaze-type chart, however, it is seen that following Type 1 there is no representation whatever of Types 2, 3, and 4, Types 5 and 6 coming in abruptly at a relatively high per cent. This shows that there was no deposition whatever of refuse, from some time during the life of Type I, until the comparatively late date when glaze as a trait was on the decline, and the late wares had already come into vogue (see curves for modern, polished black, and plain red).[1] We have, then, in test XIII a long section of pure Type I debris, making clear the rise and development of that type, and providing helpful knowledge as to its relation to the precedent black-on-white and the contemporaneous phases of biscuit. This test proves, for example, a thing which we had not hitherto suspected, *viz.*: that biscuit *B* came into limited use before the origin of Type 2 glaze.

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[1] It may be considered axiomatic that the faster a heap accumulates the purer and less disturbed will be its contents.

We cannot, within the limits of this paper, and with the scanty material now at hand, go further into detail; but enough has been said to show the value of test analysis and the importance of plotting the results graphically.

RECAPITULATION

The earliest ware found on the Pecos mesa is the black-on-white. With it occurs blackware of the strong blind-corrugated variety and a small percentage of true corrugated, but never as far as our investigations have shown, is there any indented corrugated. Further, the dull-black-on-red and the black-on-white with basket-marked exterior, both classes noted by Mr. Nelson from Tano ruins and by the author from the lesser sites on the Pajarito and near Santa Fe,[2] are not represented at Pecos. They are to be found, however, at "Bandelier Bend," a ruin a little below Pecos on the opposite side of the arroyo, and at several other places near Glorieta. As the corrugated technic gradually fades out at Pecos from the early strata upwards, the most strongly marked specimens there occurring at the bottom, it is reasonable to suppose that the ruins which produce the best grades of the ware are earlier still. Hence we conclude that the Pecos mesa was not settled until toward the close of the black-on-white period. Whether or not glazing had been discovered or introduced at that time is still an open question. The great preponderance of black-on-white pottery in the early strata prove that glazing could not have attained any considerable vogue, yet the lowest cuts in all our tests show traces of glazed pottery. On the other hand, isolated rubbish heaps of the north terrace, and a black-on-white cemetery on the west slope, contain no glazed sherds at all. We believe, therefore, that for a short time at least, the black-on-white industry was the only one practised at Pecos.

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[1] As an illustration of the purely archaeological value of test analysis it may be said that the lacuna in this particular rubbish heap throws very important light on the method of growth of the north end of the Pueblo.

[2] "*N*," p. 171; "*K*," p. 414.

When glazing did come in it very quickly drove out the older style. This is clearly shown by the charts; they indicate, however, that glazing and black-on-white existed for a short time side by side. This is of course only natural, for we have no reason to think that there was an abandonment of the pueblo during the transition, or that any wholesale influx of outside clans brought the change about abruptly.[1]

Glazing was probably not of simultaneous origin in the whole country. It was undoubtedly discovered accidentally at some one site by the mixing of a flux in the pigment, and spread outward from there, the novelty and beauty of the paint aiding rapid dissemination. While it is of course futile to try to locate the point of origin with any exactitude, we believe that it lay to the west, probably in the Little Colorado; our reasons for this belief are as follows: The earliest form of glazing in the Rio Grande was the black-on-red. This has been pointed out by Mr. Nelson, and is confirmed by the Pecos finds. Now black-on-red is exceedingly rare in dull-paint pottery of the Rio Grande black-on-white period; and, if our Pecos observations are correct, it had entirely disappeared during the later phases of that period. Hence, it seems strange that a new paint, if discovered locally, should first have been applied over a slip of a color that was seldom if ever used locally at the time. The southern and western parts of the pueblo district seem always to have been the greatest centres for the manufacture of red wares, therefore the sudden appearance of a redware with a new form of paint in the east suggests western influence.

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[1] Clan-migration should only be called upon as a last resort to explain pottery changes, for while we know that such movements were common and a very potent factor in the makeup of all the later pueblos, we have no evidence that they were capable of producing radical overturns in resident cultures.

The above conclusion is entirely theoretical; to strengthen it, however, we have a few bits of archaeological evidence. In the Little Colorado drainage there occurs a redware with glaze decoration, whose bowls bear characteristic exterior designs in chalky white paint. Although Fewkes and Hough have figured a number of pieces, the chronological position of the style has never been fixed; the vessels, however, bear internal evidence of belonging to a fairly well-developed ceramic industry. We know of four fragments of such vessels from the Rio Grande: one from the Rowe ruin, an almost straight black-on-white site; two from Agua Fria "schoolhouse," which from its preponderance of black-on-red glazed ware we are justified in assigning to an early period; one from cut 7, Test X, a very early stratum, as it contains 25 per cent. of black-on-white ware. These were presumably all trade pieces and so serve to indicate with some degree of probability that glaze decoration was present and perhaps even fairly well advanced in the Little Colorado while it was still a very young art in the Rio Grande.

The locating of the origin of glaze to the west rests, of course, on possibly faulty theoretical grounds, and at best very meagre archaeological evidence. It is, however, a convenient working hypothesis which further research in the western districts and a close study of the designs will either prove or disprove. Whether or not glaze was first discovered in the west, it reached its maximum development in the east; and that development was, as far as we can see, a gradual and purely local one.

Returning to the Type 1 glazed wares, we may repeat that the black-on-red phase was at first the preponderating one. The black-on-light, however, slowly gains in importance and, as we approach Type 2, the red falls away. Type 2 is marked by a degeneration of the glaze, which becomes thicker and less adaptable to the production of fine-line designs; by a parallel deterioration of the clear yellow color of the slip; by a thickening inwards of the bowl rim (which, however, still remains square on top); and by the appearance on bowl exteriors, first of slashed marks and crosses, then of red-filled decorations. The transitional nature of Type 2, both technologically and stratigraphically, was pointed out in the preceding section. Although the ware is less uniformly specialized than those of our other groups, it appears at Pecos in large quantities, indicating that its manufacture was carried on during a considerable period of time; further, the ware is characteristic of at least one isolated site which contains nothing else.[1] For these reasons we have given it typical rank.

The other wares associated with Types 1 and 2 glaze are the black and the biscuit. Of the former little need be said; it shows a steady elimination of the visible signs of the structural coil and a progressive modification of the rim. Biscuitware presents several interesting features. Biscuit *A*, the early variety, is unslipped on bowl exteriors and bears decorations which seldom if ever conform to the highly specialized design-system of biscuit *B*; but which seem, on the basis of the sherds, to be allied rather closely to those of the black-on-white. That the ware is a development from the black-on-white would naturally suggest itself, and indeed seemingly transitional pieces are not rare. The change from biscuit *A* to biscuit *B* consists in the raising and straightening of the bowl-rim, in the specialization of the design, and in its extension with the slip to bowl exteriors. In general, biscuit *A* is associable with Type 1 glaze, *B* with 2, 3, 4, and very early 5.[2]

Returning to the development of the glazed wares, we see that in Type 3 the slip on visible surfaces is almost invariably light, but is of poor and uncertain quality; the glaze lines also are seldom sharp-edged and clear black, but are heavy, "runny," and of various shades of rusty black, brown, and greeny-brown. Because of the intractability of the paint the interior designs of bowls and the body designs of ollas are considerably simplified; the exteriors of bowls and the necks of ollas, on the other hand, bear handsome decorations (usually oblique variants of the double-ended key figure) in red with glaze outlines. Type 5 seems to be merely a further growth along the same lines, *viz.*: glaze becomes even heavier;

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[1] The "Frijolito" ruin, see "*K*," p. 454.

[2] Test XIII shows biscuit *B* to have originated a little earlier than Type 2 glaze.

interior decoration of bowls is simplified to a mere pair of red filled lines encircling the piece just below the lip; exterior design grows more varied; and finally the rim reaches a maximum of thickness.

Found in the same strata that produce the latest forms of Type 3 and the earlier forms of Type 5, is a certain percentage of pieces scarcely, if at all, different from the pottery which in a former paper we have called "Pajaritan,"[1] and have described as Type 4 in the present one. It seems to come in abruptly, runs but a short course, and abruptly disappears, apparently with no preceding or succeeding type. Its high rather thin rims, red color, good glaze, and specialized decoration, do not fit at all well in the Pecos series. It was commonly buried with the dead, as were Types 1, 2, and 3; while Type 5 vessels were almost never so deposited. One of three conclusions might be arrived at:[2] (1) that it was really transitional between 3 and 5, but that we have not sufficient evidence or sufficient acumen to enable us to recognize it as such; (2) that it was a short-lived secondary style, arising from Type 3 and running a course parallel to early Type 5; (3) that it was made by a group of people from the Pajarito who settled at Pecos, as the Hano did at Hopi, were assigned a section of the site, made their own style of pottery, and continued their own mortuary customs; finally losing their cultural identity or moving away. Careful work in the rubbish will, without much doubt, eventually settle this interesting problem.

The wares found in conjunction with Type 5 glaze are the featureless black and, towards the end, the striated black; with its earlier phases, or roughly synchronous with the Pajaritan admixture just noted, there is a considerable amount of biscuit *B*, which seems to average yellower than the earlier biscuit *B* and to have higher rims. This ware rapidly decreases in importance and, shortly after the middle of the Type 5 glaze period, it entirely disappears.

Type 5 continued well into historic times. This is proved by the finding in Type 5 rubbish of the bones of domestic animals and objects of European manufacture. At just what period in the life of Type 5 the Conquest occurred cannot yet be stated, as the bones and artifacts from the tests have not yet been studied, nor can we as yet put a date to the appearance of Type 6, the last stage of native glazed ware.

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[1] "*K*," p. 417.

[2] Specimens are too common to be considered trade pieces.

Type 6 is distinguishable from the preceding one by the diversity and eccentricity of the vessel shapes, and by the extremely "runny" nature of the glaze. We quote Mr. Nelson's excellent description:

> Generally the iridescent glaze substance is of such striking and excellent quality as to incline one to believe that it was compounded after a Spanish formula. The fact that the artist could not control it at all seems suggestive of the same idea. The designs attempted, though of the very simplest geometric nature, were almost invariably spoiled by the running of the glaze. The color and general appearance is a very characteristic dark brown when thickly applied and of a greenish hue when the coating is thin.

The fact that the ware is all of historic date lends much weight to Mr. Nelson's supposition that the glaze was compounded after Spanish formulae. If such was the case, the style must have been a very short-lived one indeed, as Spanish influence strong enough to have seriously affected the manufacture of pottery can hardly be believed to have exerted itself prior to the actual settlement of the country at the beginning of the seventeenth century, and Mr. Nelson has shown that the making of glazed pottery ceased at about the time of the revolt of 1680.[1]

Next came in the dull-paint vessels described above as "modern-painted ware." Their whole appearance, color, pigments, and decorative elements are radically different from those of glazed pottery. The only resemblance is in the high, outcurved rim that is common both to modern and to glaze Type 6. As to the rapidity of the rise of modern ware we cannot yet supply much data, as our tests all tend to become unreliable toward the top. That it eventually completely superseded glazed ware is proved by the total absence of the latter from certain isolated heaps of late rubbish found at Pecos, and by Mr. Nelson's observations at Cienega no. 1 and Cieneguilla.[2] The origin of the style is as yet a mystery. On its first recorded appearance it bears designs that by no stretch of the imagination may be believed to be the direct descendants of any of the glazed designs. Technically it is most nearly allied to biscuitware, and some of the decorative elements may possibly be derived from biscuit prototypes. If it is a late outgrowth of biscuit, that growth can hardly have taken place at Pecos, for our test material shows that the making of biscuit had probably ceased completely some time before the modern came into vogue. Mr. Nelson throws out the hint that the type may have been derived from the Keresan region to the north, and this is a particularly useful suggestion in view of the fact that biscuitware was primarily a northern product, reached its maximum development in the upper reaches of the Rio Grande and the Chama, and seems to have continued in use in that district rather longer than it did in the central Rio Grande. It is possible, therefore, that we may yet find that modern painted ware was a normal growth from an ancient prototype, probably biscuit, in some region to the north, and that the unsettled conditions of the rebellion, with their constant movements of people, may have brought about its introduction to the central Rio Grande as an already perfected style. If this is so, we will not be forced to postulate a conscious revolt against what Mr. Nelson calls "the degenerative tendency of the seventeenth century"; or to believe, as we have sometimes done, that the modern ware was the result of an archaistic revival carried out by the Pueblos at the time of the rebellion in a desire to do away with everything savoring of Spanish influence, and to return to the dull-painted wares that observation of the sherds about their villages must have shown them were made by their prehistoric ancestors

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[1] "*N*," p. 175. The Pecos ruin can hardly be expected to add corroboration to this, as the site was not deserted at any time during the rebellion.

[2] "*N*," p. 176 and table p. 179.

That there were changes in the modern ware after its introduction and continuing until the time of the abandonment of Pecos in 1838, can be seen by examining the pottery fragments in and around the houses on the mesa-top. Similar changes evidently also took place in the polished black and the plain red. Late debris, however, in large enough quantities for satisfactory tests, has not yet been found lying conformably upon earlier strata. Such deposits surely exist at Pecos, but until they have been located and examined it is idle to attempt to carry the study of type-sequence beyond the end of glaze-making times.

TENTATIVE DIVISION OF THE CULTURE-HISTORY OF THE EAST-CENTRAL RIO GRANDE INTO CHRONOLOGICAL PERIODS (the leading ware of each period is italicized).

I. Formative period.	Wares?	Nelson's pre-pueblo, not yet found in Pecos valley.
II. Archaic period....	Indented corrugated, *black-on-white*, black-on-red.	Nelson's I. Not found at Pecos, but present at other nearby sites.
III. Late archaic......	Corrugated (little), strong blind-corrugated, *black-on-white*.	Nelson's I.
IV. Period of introduction of glaze.[1] . .	Strong and medium blind-corrugated, *glaze 1*, biscuit *A*.	Nelson's II. Author's Agua Fria "schoolhouse."
V. Period of concentration.[2]	Faint blind-corrugated, *glazes 2 and 3*, biscuit *B*.	Nelson's III. Author's "Frijolito."
VI. Late prehistoric. ?–1600........	Featureless black, glaze 4, *glaze 5*, biscuit *B*.	Nelson's III. Author's "Pajaritan."
VII. Early historic (1540) 1600–1680..........	Striated black, late glaze 5, *glaze 6*, modern begins (?).	Nelson's IV.
VIII. Late period 1680–1840..........	Striated black (?), plain red, polished black, *modern*.	Nelson's V.
IX. Present period 1840–1917......	Polished wares (Santa Clara), *painted wares* (San Ildefonso, Santo Domingo, Tesuque, Cochiti).	

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[1] And possibly also of the kiva.

[2] Concentration had been going on from the earliest times, but apparently became particularly strong at about this time in the Pecos valley and on the Pajarito. In the Tano country it took place somewhat later (see Nelson's chart, p. 179), perhaps because the Galisteo basin was less exposed to raids from the north and east.

ORIGIN OF THE WORD SHAMAN

By BERTHOLD LAUFER

IN *Chinese Clay Figures* (p. 198) the writer had occasion to discuss briefly the history of our word shaman, and to refer at the close of his naturally succinct note to the new theory of J. Németh, according to which the term should be an ancient constituent of the Turkish-Tungusian languages. The latter reference was an addition inserted in the proofs at the moment when Mr. Németh's article reached me (summer 1914), but since then it has been possible to scrutinize his theory at close range, with the result that it can be confirmed and even be supported with new data and arguments. As ethnologists may be expected to take an interest in the history of a term which has become part and parcel of anthropological nomenclature, and the origin of which has given rise to numerous discussions and speculations, a brief examination of the case might not be unwelcome to the readers of the *Anthropologist.*

There could never be a serious doubt of the real source from which the word shaman is derived. We received it from the Russians: it was the Russian (chiefly Cossack) explorers and conquerors of eastern Siberia in the second half of the seventeenth century, who heard and recorded the term among Tungusian tribes. It was first brought to Europe by the Hollander, E. Ysbrants Ides, and by Adam Brand, who from 1692 to 1695 accompanied a Russian embassy sent by Peter the Great to China. Some examples from the former's *Driejaarige Reize naar China*, first published at Amsterdam, 1698 (again 1704 and 1710), may be cited:

> Eenige dagen reizens van hier, is de groote steenachtige waterval, Schammanskoi, of Toverval geheten, om dat aldaar een beroemde Schamam, of Tungusche duiveldienaar woont (p. 34).
>
> Eenige mylen van hier opwaarts woonen veele Tunguzers, waar onder ook hun beroemde Schaman, of Duivelskonstenaar (p. 35; follows a lengthy description of the shaman and his costume).
>
> Zy [the Tungus] weeten van geen andere Priesteren, dan hunne Schamannen, of Duivelbanners (p. 39).

It is thus shown that our "shaman" had its origin in the Tungusian languages, in all of which the word indeed is known as *šaman* or *saman*, and with several other variations to be noted hereafter. It would be reasonable to regard the term as of native origin, for it is not likely that the name of a religious institution and function so characteristic of all the tribes of northern Asia should be borrowed from an outside quarter. Nevertheless it was possible that at a time when little was known about Siberian shamanism, and when rigid philological principles were not yet established, the theory could be advanced that Tungusian *šaman* or *saman* were to be derived from Chinese *ša-men*, itself a transcription of Pāli *samaṇa* corresponding to Sanskrit *çramaṇa*, a technical term for the designation of a Buddhist monk or ascetic. The germ of this idea was well prepared as early as the eighteenth century. The Augustinian Pater A. Georgi[1] states correctly that the Samanaei mentioned by Clemens Alexandrinus[2] are the adherents of Buddha, and in this connection refers to La Croze, who already hazarded the combination of the Samanaei with the shamans of the Tungusians ("le nom de schaman est un nom de religion d'un peuple tartare, voisin des frontières de la Chine"). Sanskrit not yet being well known at the time of La Croze and Georgi, the origin of the Greek term Samanaioi escaped them; this is what the philologists of the nineteenth century supplied.[3]

Which of them was the first to assert the connection of the Siberian *šaman* with the Indian term I am unable to ferret out; this point is immaterial, as the question is merely that of the history of an error. D. Banzarov[4] was inclined, without giving a specific reference, to fix the responsibility on F. Schlegel, according to whom the *çramaṇa* among the crude tribes of Central Asia transformed themselves from hermits into shamans. I believe that Banzarov had in mind Friedrich von Schlegel (1772–1829), romantic poet and Sanskrit scholar, who published in Heidelberg, 1808, an essay under the title *Über die Sprache und Weisheit der Indier, ein Beitrag zur Begründung der Alterthumskunde.*[1] This is the only one of his works in which such a statement might be supposed to occur; but on a cursory perusal of this book I am unable to trace it. The idea itself being romantic, there would be no reason to wonder that it took its root in the Romantic School. At all events it was alive in the first part of the nineteenth century. The French sinologue J.-P. Abel-Rémusat (1788–1832) became a champion of this theory.[2] An ingenious scholar of the type of J. Klaproth[3] was capable of saying, "Les peuples tartares ont sans doute reçu le mot *chaman* de l'Inde avec le bouddhisme, car il est indien d'origine, et signifie un homme qui a vaincu toutes ses passions." In 1857 Max Müller[4] of Oxford wrote,

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[1] *Alphabetum Tibetanum* (Rome, 1762), p. 222.

[2] In fact Clemens avails himself of the form Sarmanai.

[3] Maturin Veyssiere La Croze was born at Nantes in 1661, joined the clergy, suffered persecution on account of his liberal views, fled to Germany, where he turned a Protestant in 1696, and was made a member of the Academy of Sciences of Berlin, where he died in 1739. His work *Histoire du Christianisme des Indes*, to which Georgi refers, unfortunately is not accessible to me. La Croze was a scholar of wide knowledge and linguistic ability: he was the first who had a presentiment of the relationship of Indian with Persian.

[4] *Černaya v'ĕra*, p. 34 (see p. 366).

> Shamanism found its way from India to Siberia *viâ* Tibet, China, and Mongolia. Rules on the formation of magic figures, on the treatment of diseases by charms, on the worship of evil spirits, on the acquisition of the supernatural powers, on charms, incantations, and other branches of Shaman witchcraft, are found in the Tanjur, or the second part of the Tibetan canon, and in some of the late Tantras of the Nepalese collection.

Needless to add that the magical writings of the Tanjur are unknown to the shamans of Siberia and have nothing to do with Siberian shamanism. Still less is it intelligible how Müller could say that "the only trace of the influence of Buddhism among the Chudic races, the Finn, Lapp, etc., is found in the name of their priests and sorcerers, the shamans." The word shaman is foreign to any Finno-Ugrian language; it found its way into Finnish (*shamani*) and Hungarian as a scientific term only, in the same manner as into the Indo-European languages. A. H. Sayce[1] has the following:

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[1] An English translation was edited in London, 1889, in *F. v. Schlegel's Aesthetic and Miscellaneous Works.*

[2] *Recherches sur les langues tartares* (Paris, 1820), p. 133, and *Observations sur la doctrine samanéenne et la triade suprême* (Paris, 1831).

[3] *Mémoires relatifs à l'Asie* (Paris, 1828), vol. III, p. 67.

[4] *Chips from a German Workshop*, vol. I, pp. 233–234.

> In shamanism, so called from the Shaman or Siberian sorcerer, who is himself but a transformed *çramaṇa*, or Buddhist missionary priest, we rise to a higher conception of religion.

These superstitions were tenaciously upheld under the influence of the romantic movement of pan-Indianism, which held the minds of scholars enthralled in the first part of the nineteenth century, and the germs of which are not yet entirely exterminated.

True it is that the Sanskrit word *çramaṇa* with the Prākrit form *samaṇa* has become widely known outside of the pale of India. It reached the ears of the Greek writers on India, who described the Buddhist monastics under such names as Sarmanes, Sarmanai, or Samanaioi. The first of these names was recorded by Megasthenes,[2] though the single text of Strabo from which all the existing codices have been copied offers the erroneous transcription Garmanes.[3] The word has also passed into early Arabic records for the designation of the Indian and Bactrian Buddhists (for instance, in Masūdi, Gurdezi, and Abu'l Faradj) in the form *samaniyya*, while in later literature the form *šaman* was adopted.[4] In Firdausī's Shāhnāme, completed in A.D. 1010, we find *šaman* in the sense of a worshipper of idols, and this Persian word likewise is traced to Sanskrit *çramaṇa* and its congeners.[5] Again, it bears no relation to "our" or the

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[1] *Introduction to the Science of Language*, vol. II, p. 293.

[2] Strabo XV, I, 59.

[3] The texts in question are easily accessible in J. W. McCrindle, *Ancient India as described in Classical Literature*, pp. 65, 168, 170. See further C. Lassen, "De nominibus quibus a veteribus appellantur Indorum philosophi," in *Rhein. Museum für Philol.*, vol. I, 1833, pp. 178–180, and *Indische Alterthumskunde*, vol. II, p. 700; E. Burnouf, *Introduction à l'histoire du Buddhisme indien*, p. 245; T. Watters, *Essays on the Chinese Language*, p. 403; S. Lévi, *Journal asiatique* (1911), I, p. 445.

[4] Geza Kuun, *Keleti Szemle*, vol. IV, 1903, p. 133; G. Ferrand, *Textes géographiques arabes relatifs à l'Extrême-Orient*, vol. I, p. 131. The Mohammedan writers were perfectly conscious of the fact that the term hailed from India and denoted the Buddhists of India and China. See particularly D. Chwolsohn, *Die Ssabier und der Ssabismus* (St. Petersburg, 1856), vol. I, pp. 165, 214, 217, etc. As far as I know, the shamanistic religion of Siberian tribes is not mentioned by any early Arabic or Persian author.

[5] P. Horn in *Grundriss der iranischen Philologie*, vol. I, pt. 2, p. 7; K. Vullers (*Lexicon persico-latinum*, vol. I, p. 466) renders the term by *idololatra* and *idolum*.

Siberian shaman, but evidently refers to Indian Buddhists, for these šamans say, "Our religion is one of peace and quiet, and fighting and slaying is prohibited, as well as all kinds of shedding of blood."[1] Finally the Indian word has been traced in the newly discovered Indo-European language styled Tokharian in the form *ṣamāne;* and this induced A. Meillet to formulate the opinion that the Tokharian form (not a Prākrit form of Sanskrit *çramaṇa*) would account for the Tungus word *šaman.*[2] With all respect due to the scholarship of this eminent philologist it must be admitted, however, that there is a far cry from Tokharian to Tungusian, that there is no evidence for any contact of these two groups, and that the Tungus notion of a shaman is radically different from the conception of a Buddhist monk. If two words in geographically remote languages are physically alike or similar, but fundamentally diverse in meaning, it is safe to assume that the face resemblance is purely accidental.

Indeed this theory was brilliantly antagonized as early as 1842 by W. Schott; and H. Yule,[3] accepting his verdict, reached the conclusion, "Whether the Tungus word is in any way connected with this or adopted from it [that is, the Sanskrit-Pāli term], is a doubtful question." Schott has devoted no less than three studies to this problem,[4] in which he endeavored to dissociate *šaman* from *samaṇa.* His arguments are briefly as follows: the Chinese strictly discriminate between Manchu *saman* (shaman) and Sanskrit *çramaṇa,* and never confound a shaman and a Buddhist monk, the two having totally diverse functions; a direct or indirect connection of Tungusian tribes with India is unproved; Buddhist missionaries never advanced into regions inhabited by Tungus;[5]

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[1] Compare the quotation in H. Yule, *Hobson-Jobson,* p. 820.

[2] A. Meillet, "Le Tokharien," *Indogermanisches Jahrbuch* (Strassburg, 1914), vol. I, p. 19.

[3] *Hobson-Jobson,* p. 820.

[4] "Über den Doppelsinn des Wortes Schamane und über den tungusischen Schamanen-Cultus am Hofe der Mandju-Kaiser," *Abhandlungen Berliner Akademie,* 1842, pp. 1–8; "Wohin gehört das Wort Schamane?" in his Altaische Studien, no. 3, *ibid.,* 1867, pp. 138–141; and "Das Wort Schamane," in *Erman's Archiv für wissenschaftliche Kunde von Russland,* vol. XXIII, 1865, pp. 207–210.

[5] As emphasized in my article "Burkhan," *Journal American Oriental Society* (1917), pp. 390–395, there is no trace of Buddhism in the religion of the Tungusian peoples on the Amur.

it would be strange if they should have borrowed the only word in their language for the designation of their national priests; the forms of shamanism are uniform in a vast territory extending from Lappland to the Bering Sea and far beyond into America; Vasilyev's hypothesis that shamanism should be a caricature of ancient Buddhist sorcery and jugglery and have been brought into existence by such influences is a mere phantom; the outward resemblance of the words *saman* and *samaṇa* is purely fortuitous. Schott's etymological explanations of the word from Samoyed are not convincing, but his remark that Tungusian *sam* possibly is anciently related to Turkish *kam* testifies to great foresight and ingenuity; and this thesis, as will be seen, can now really be proved. Also Ch. de Harlez[1] joined the ranks of the adversaries of the Indian hypothesis, reiterating in the main Schott's arguments. In Russia, it had been severely criticized as far back as 1846 by Dordji Banzarov, a Buryat and an excellent Mongol scholar.[2] Nevertheless in Russian dictionaries also the nightmare of the *çramaṇa* is perpetuated.[3] As it happens, twilight reveries appeal more to the multitude than plain reason and continue to live even if abandoned by the thinking minority; it is not always the best and fittest that survives. The ghost of the Indian *çramaṇa* still haunts the poor shaman in our standard dictionaries and cyclopaedias (see Webster, Nouveau Larousse, and Littré, *Dictionnaire de la langue française;* Schaff-Herzog *Religious Encyclopaedia*, vol. x, p. 385; Funk and Wagnalls' Dictionary offers even a "Persian-Hindustani shaman, pagan"!).[4] It is a relief, therefore, to read in the new Oxford Dictionary,

> The Persian *shemen*, idol, idol temple, sometimes cited as the source, is unconnected. Evidence seems to be wanting for the plausible suggestion that the Mongolian word is an adoption of Chinese *sha men*, an ordained member of a Buddhist fraternity.

Eliminating "plausible" and substituting "Tungusian" for

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[1] *La Religion nationale des Tartares orientaux* (Bruxelles, 1887), p. 28.

[2] His works were collected and edited by Potanin in 1891 under the title *Černaya v'ära ili šamanstvo u Mongolov i drugiya stat'i* ("The Black Faith or Shamanism among the Mongols and Other Articles"). Regarding the word shaman see p. 34.

[3] For instance, in Gor'aev, *Sravnitelny etimologičeski slovar' russkago yazyka*, p. 418.

[4] Even in the 7th edition of O. Peschel's *Völkerkunde* (p. 274) the *çramaṇa* still holds sway.

"Mongolian," this statement marks a progress in lexicographical literature. So there is hope that our great-grandchildren will perhaps be treated to a correct definition in the dictionaries of the following century.

The Hungarian scholar J. Németh[1] has attacked the problem in the right spirit, and ably solved it by means of a sane philological method. A schooled phonetician, he has successfully worked for several years on the phonetic history of the Turkish and Mongol languages and established very interesting correspondences of phonetic phenomena in these two linguistic families. In the study cited he advances the law that primeval Turkish initial *k'* (palatalized *k*) has developed into Tartar *k*, Čuvaš *j*, Yakut *x* (spirant surd as in German *ach*), Mongol *ts*, *č*, and Manchu-Tungusian *s*, *ś*, or *š*. He gives seven examples to this effect, the first of which is:

> Turkish (Uigur and in the dialects of southern Siberia) ***kam*** or anciently ***qam*** (shaman), in some dialects ***küär***, ***kum*** (with such derivations as ***kamdi***, to prophesy; ***kamla***, to heal; ***kamna***, to shamanize; etc.). Čuvaš ***jum***, ***jumeš*** (shaman); ***im-jum*** (curative charms). Manchu ***saman***, Golde ***šama***, ***š'ama***, Olča ***šam***, Oročon ***s'ama***, Tungus on the middle Amur ***samaṅ***, Tungus of Udsk ***saman*** and ***šaman***, Kondogir and Wilui Tungus ***xaman***.

Németh concludes that the origin of the much disputed word "shaman" is thus definitely solved, and that it is an ancient property of the Turkish-Mongol languages. The phonetic alternation determined by him is perfectly correct; and Manchu-Tungusian *sam*, *s'am*, or *šam*[2] is indeed the exact phonetic equivalent of Turkish *kam*.[3] Apart from the Tungusian series noted by Németh after Grube's *Goldisches Wörterverzeichnis*, several other forms of the word have become known. De la Brunière[1] recorded it in 1846 among the Golde as *tsama* or *tsamo*. I heard only *s'ama* from the Golde, in the same manner as Shimkevič,[2] but in view of the existence of several Golde dialects such a form may be regarded as possible. A. Rudnev[3] noted from a Manchu, who was still conversant with his mother tongue, the following: *sáma*, *sáman*, *sámą* (nasalized *a*), and *sāmăn*.

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[1] "Über den Ursprung des Wortes Šaman und einige Bemerkungen zur türkisch-mongolischen Lautgeschichte," *Keleti Szemle*, vol. XIV, 1913–14, pp. 240–249.

[2] This in fact is the stem of the word, not as formerly believed by some scholars, *sa-*, which was explained from Manchu *sa-* (to know). Németh justly rejects this derivation. By the way, Schott, the father of this etymology, disavowed it himself in his second treatise on the word shaman, quoted above. D. Banzarov (*l. c.*, p. 35) justly recognized *sam* as the stem.

[3] It was hitherto believed that the word for shaman was different in all languages of Siberia, while there is a fairly uniform designation for the female shaman (*udagān*, *utygan*, *ubakxan*, etc.). Hence V. F. Troščanski (Evolutsiya černoi v'äry [šamanstva] u Yakutov = "Evolution of the Black Faith among the Yakut," Kazan, 1902, p. 118) was prompted to the theory that the Siberian tribes originally had only female shamans, and after the time of their separation instituted also male shamans. This reconstruction is contestable on many grounds; for instance, we have male and female shamans

The Gilyak style the shaman *čam* (*tšam*). This certainly is a loan-word received from Tungusian,[4] but it is not necessary to assume that the change of the initial *š* or *s* into the palatal explosive surd is due to the Gilyak; for this alternation is common within Tungusian, so that a Tungusian *čam* borrowed by the Gilyak may be presupposed for some Tungusian dialect spoken in the neighborhood of the Gilyak. Compare the following examples: Manchu *nesuken* and *nečin* (equal)—Jučen *nušin;* Manchu *ničuxe* (pearl)—Old Jučen *inšuko* or *inšuxa*, modern Jučen *ninčuxe;* or *vice versâ*, Jučen *ančun* (gold)—Manchu *aisin*.

It has been said that the word *šaman* is found only in the eastern part of Siberia and in Manchuria. There is, however, one well-authenticated instance on record to the effect that it was known also in northwestern Siberia. In the earliest account that we have of the Irtysh-Ostyak, written in 1715 by G. Novitski, their priests were styled *šamančik*.[5] In view of the early date of this record we cannot well assume that this is a loan-word received through intercourse with the Russians. Whether the term still exists I

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co-existent in the most ancient days of China. The supposition on which the conclusion is based is now no longer valid: the word for the male shaman is in fact identical throughout from the Turkish to the Tungusian and Gilyak tribes.

[1] In H. E. M. James, *The Long White Mountain or a Journey in Manchuria* (London, 1888), pp. 433, 434.

[2] *Materialy dl'a izučeniya šamanstva u Goldov*, p. 8.

[3] *Novyia dannyia po živoi mandžurskoi r'äči i šamanstvu* (*New Data on the Live Manchu Speech and Shamanism*), p. 9.

[4] There is no doubt that the shamanism of the Gilyak is derived from and a weak echo of that of the neighboring Amur tribes. There are but a few shamans among the Gilyak, and shamanizing is rarely practised by them (see for the present L. von Schrenck, *Reisen und Forschungen im Amur-Lande*, vol. III, p. 752).

[5] B. Munkácsi, *Keleti Szemle*, vol. IV, 1903, p. 88.

am unable to say from the sources at my disposal. S. Patkanow[1] gives for the shamans of the Ostyak only the name *tonx-ort*.[2]

The Turkish word *kam* is of ancient date.[3] It is attested (and this is its earliest occurrence) in the Chinese Annals of the T'ang dynasty (618–906), where it is said with respect to the Kirgiz[4] that they designate their sorcerers or shamans (Chinese *wu*) with the word *kam* 呼巫爲甘. The last character now reads *kan*, but was anciently possessed of a final labial nasal, which is still preserved in the dialects of southern China. Chinese *wu* is a very ancient term for the native medicine-men.[5] The word *kam*, further, is found in Uigur[6] and in the *Codex cumanicus* of 1303.[7] It is also used by the Persian historian Rashid-eddīn (1247–1318) in 1302, with reference to the shamans of the Mongols.[8] The existence of the Tungusian word *šaman* can now be traced to the first part of the twelfth century, for Pelliot[9] has found it as a Jučen word in a Chinese gloss, where it is transcribed in the form *šan-man* and explained as a sorceress. The Chinese transliteration would presuppose a Jučen form *šamman*. Indeed we still meet forms with double *m* in the dialect of the Wilui Tungus,—*samman* and *xamman*.[10] The latter with initial spirant represents the missing link between Turkish *kam* (in the language of the Soyot or Urangkhai *xam*) and the Tungusian forms with initial sibilant. The alternation of *k* and *x* is well known;[1] likewise *x*, *h*, and *s*, alternate in Tungusian dialects.[2] For this reason it is justifiable to correlate the forms *kam* and *xam* with Yakut *xamnā*, *xamsā* (to move, to be agitated; *xām*, to step, to stride),[3] and to interpret the significance of the word from the shaman's peculiar behavior, his solemn and pompous pacing, his ecstasy, his convulsions, his frantic leaps and dancing.[4]

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[1] *Die Irtysch-Ostjaken*, pt. I, p. 120.

[2] Consulting Castrén's vocabulary in his *Versuch einer ostjakischen Sprachlehre* I find that *tonx* means idol, and *ort*, workman, slave. If accordingly *tonx-ort* should mean servant of the idols, the term might hardly be claimed as a primeval designation of the shaman.

[3] The Russians have derived from it the verb *kamlat'* (to shamanize, to conjure, to spell, to prophesy) and the noun *kamlanie* (the act of shamanizing).

[4] Regarding this name and its ancient Chinese transcriptions see Pelliot and Laufer, *T'oung Pao*, 1916, p. 295.

[5] Regarding their activity see *Chinese Clay Figures*, pp. 198–200.

[6] In the Kudatku Bilik, written in A. D. 1068 (H. Vámbéry, *Uigurische Sprachmonumente*, p. 221).

[7] W. Radloff, *Aus Sibirien*, vol. II, p. 67. Németh overlooked these data, but it is important to note the wide distribution of the word in time and space.

[8] F. von Erdmann, *Temudschin*, p. 553; E. Blochet, *Introduction à l'histoire des Mongols de Rashid Ed-Din*, p. 165.

[9] *Journal asiatique*, 1913, mars-avril, pp. 466–469.

[10] Recorded by R. Maack, *Wil'uski okrug* (*The District of Wilui*), pt. 3, p. 116.

Németh gives no form of the word for Mongol, and explains the absence of a corresponding word in this language through a characteristic trait of the Mongols to whom the use of many words was tabooed; owing to such a taboo, the word *šaman* should have been lost. This is an arbitrary, far-fetched, and unnecessary theory. First of all, the word *šaman* does occur in Mongol, at least it is listed as a Mongol word in Kovalevski's and Golstunski's Mongol dictionaries. Whether it is frequent is another question. I never found it in a Mongol text. This is accounted for by the fact that the common term for a shaman in Mongol is *bügä*, *bögä*, Buryat *buge* and *bȫ*, corresponding to Turkish *bȫgü*, Djagatai *bügi* (sorcery), *bügiči* (sorcerer), Osmanli *büi* (sorcery).[5] This series seems to bear some kind of relation to Chinese *bu*, *wu* (shaman), *buk*, *puk* (to divine), and Tibetan *ạba* (pronounced *ba*, sorcerer);[6] but how this relation is to be explained is beyond our present knowledge. The type *kam—sam* is possibly preserved in Mongol in a somewhat disguised form. According to the phonetic law established by Németh, the Mongol initial corresponding to Turkish *k* and Tungusian *s* should be *ts*, *č*. Thus we get at Mongol *tsam*, *čam*, that means a dance, and at present refers in particular to the religious pantomimic dance-performances of the Lamas in which, as is well known, many ancient shamanistic notions and rites of pre-Buddhistic times have survived. The same word in the same sense is encountered in Tibetan as *č'am* or *ts'am* (written *ạč'am*), and it would be justifiable to regard the Mongol as a loan-word adopted from Tibetan;[1] but considering the fact that the Tibetan word is isolated (that is, not found in any other Indo-Chinese language), it is likewise permissible to stamp the Tibetan as a Mongol loan. For the rest, I give with all reserve this theory of a possible connection of Mongol *tsam* with Turkish-Tungusian *kam—sam* (religious dancer).

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[1] W. Radloff, *Phonetik der nördlichen Türksprachen*, p. 108.

[2] A. Schiefner, *Mélanges asiatiques*, vol. VIII, 1877, pp. 338–339.

[3] O. Böhthingk, *Jakutisches Wörterbuch*, p. 79.

[4] This explanation was first given by V. F. Troščanski, *Evolutsiya černoi v'äry (šamanstva) u Yakutov* (*Evolution of the Black Faith among the Yakut*), (Kazan, 1902), p. 117. D. Banzarov (*Černaya v'ära*, p. 35) held a similar view, but arrived at it through a different process by connecting the word with Manchu *samarambi* and Mongol *samoromoi*, *samagu;* aside from the fact, however, that these words do not have the meaning required, or forced into them by Banzarov, they can by no means be reduced to the stem *sam*.—*Kam* is not a Yakut word for the shaman, as wrongly stated by A. van Gennep ("De l'emploi du mot 'chamanisme,'" *Revue de l'histoire des religions*, vol. 47, 1903, p. 52); the Yakut term is *oyun*.

[5] According to I. J. Schmidt (*Geschichte der Ost-Mongolen*, St. Petersburg, 1826, p. 416), *kami* should be the Mongol name of the shamans. It would hardly be necessary to point out this blunder, had it not been immortalized in Yule's *Hobson-Jobson* (p. 820). There is no Mongol word *kami* with such a meaning. Schmidt found it in the Mongol chronicle of Sanang Setsen as the proper name of a Tibetan Bon-po, and this name reads correctly Khami or Xami. Schmidt's interpretation, evidently suggested by Turkish *kam*, is baseless and purely imaginary.

[6] Compare my concordance in *T'oung Pao*, 1916, p. 68.

At any rate it is obvious that the word *šaman* has now legitimately secured an absolute and irrevocable decree of divorce from its pseudo-mate *çramaṇa*, *samaṇa*, or *ša-men*, and that this mismated couple cannot live together any longer. Tungusian *saman*, *šaman*, *xaman*, etc., Mongol *šaman*, Turkish *kam* and *xam*, are close and inseparable allies grown and nourished on the soil of northern Asia,—live witnesses for the great antiquity of the shamanistic form of religion.

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[1] In a forthcoming study on *Loan-Words in Tibetan*, where an almost complete list of Mongol loan-words occurring in Tibetan is given, the writer has set forth the difficulties in recognizing the mutual loan-words of the two languages. Mongol and Tibetan have a certain number of words in common, partially due to a prehistoric contact, partially due to a lively intellectual interchange in historical periods; and it is not always possible to decide in each and every case which side is the recipient, and which the borrower.

THE POSITION OF THE BODY IN ABORIGINAL INTERMENTS IN WESTERN MASSACHUSETTS

By HARRIS HAWTHORNE WILDER and RALPH WHEATON WHIPPLE

SOME eleven years ago the senior author published in this periodical an account of excavations in an aboriginal cemetery in North Hadley, Massachusetts, with some details concerning the method of interment of two skeletons found there.[1] At that time the examination was made by simply uncovering the bones, and photographing them as they lay in the damp sand, noting at the time the points of the compass, and the general relations of the body. As the bones were removed separately the same day, the time allowed for study of the position was short, and the observations were made under field conditions, which are never ideal for careful work.

Since then the incompleteness of this record has become more and more apparent, and the idea has gradually developed of finding a typical aboriginal grave, boxing it up in the field, and removing it entire to the laboratory, where, under ideal conditions, the actual excavation could take place. Under such circumstances the bones could be carefully uncovered without the slightest disarrangement, and the whole interment be fixed in place through impregnation with some cementing fluid, like glue or shellac.

Although this plan was conceived in the rough soon after the excavation of 1905, its execution was delayed until the fall of 1915, when opportunity again presented itself for continuing excavation at the same site. For this the two authors, representing respectively the neighboring colleges of Smith and Amherst, associated themselves, and in the work, which has continued since this time and is still being prosecuted, the method of removing entire graves, with the objects in place, rather indefinite at first, has become so far developed by the aid of improvements that have constantly suggested themselves, as to form a special archaeological technique, which renders plain and simple certain relationships hitherto extremely difficult to study, and impossible to record permanently. For example, one may study the various mechanical causes of the displacement of the bones, differentiating the natural results of decay, with the dropping and sagging of the parts, and the complications due to the retentive force of the ligaments, from such external forces as frost, the action of earthworms, and the displacement caused by the larger burrowing animals. This being possible, the position of the body at burial, even to certain exact details, may be estimated, and such external conditions as the confinement of the limbs, or the folding up of an already stiffened body by the use of force, be satisfactorily made out.

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[1] *American Anthropologist*, (N. S.), vol. 7 (1905), pp. 295–300, with plate XXIII and a map of the site.

Aside from all this, which forms almost a distinct line of study, the simple problem of getting out the bones with the least damage is solved most satisfactorily by this method, and many a crumbling fragment of bone, or even a mass of earth which retains the shape of a bone or bone detail, hopelessly lost when working in the field, may be saved when at a laboratory table where cements, glues, and similar substances are always at hand.

It would seem, too, that this method of removing an entire grave would be of advantage in the study of neolithic graves elsewhere, especially where there are associated objects, the exact relationship of which to the body might be important. Graves of neolithic or bronze age peoples appear in many European museums, the bones and artifacts taken from the earth with care, and reassembled in a glass case as accurately as possible by the aid of photographs, drawings, and field notes. Of how much greater value would these be if it could be confidently stated that every piece had remained in its position as found at the time of excavation, without the slightest dislocation.

The present paper deals with the excavation of two aboriginal graves, dealt with in this way, and now placed in the museums of the two colleges in question; putting special emphasis on the position of the body in local interments, but incidentally illustrating the method of excavating above referred to.

The first of these was found near the Connecticut river, at North Hadley, at the site of the excavations of 1905, but farther out, upon the oval knoll shown in the map of the previous paper[1] there designated as a house-site or village. This was the traditional site of the Nonotuck chief, Quonquont, whose name appeared on several of the deeds for the meadow land, sold to that shrewd English trader from Springfield, John Pynchon. Quonquont was dead by October, 1672, but a deed of that date mentions his widow, Sarah Quonquont; a son, Pocumohouse, and a daughter, Majessit, together with two others, one being a squaw. These seem to have lived at the time at this site, and undoubtedly the village had numerous inhabitants until the defeat of Phillip in 1676 caused the abandonment of the English lands by all Indians who, like these Nonotuck, had taken part in the great confederation.

This combined site, the knoll, the field, and the neck of land connecting the two, has long been known to contain Indian skeletons, and on a number of occasions several at a time have been excavated, in excellent condition. Here the authors dug, during several days in the autumn of 1915, running about four hundred feet of exploratory trenches, and on the afternoon of November 26, a fragment of the outer condyle of a left tibia was thrown up by the shovel.

This led to the location of what proved later to be a complete male skeleton, with arms and legs folded up; beyond all doubt an aboriginal interment. The surface earth was carefully removed to within a few inches of the bones, and these were uncovered at a sufficient number of points to enable us to get the position of the skeleton, after which the whole was covered up again. As the skeleton lay in damp sand, packed down hard, there was in this process no risk of disturbing a single bone, and after the few points of exposure were covered again and the sand packed down upon them, the grave was still in its original condition, with the single exception, of course, of the condyle of the tibia, which had led to the discovery.

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[1] *loc. cit.*, p. 296.

A deep trench was then dug entirely about the grave in the form of a quadrilateral, 4 ft. by 2.5 ft., enclosing the entire grave and leaving everywhere a margin of at least six inches beyond the probable position of the bones. A frame of stout planks was then built around the grave, and the whole boarded over for the night.

The next morning a floor was driven across, just beneath the lower edge of the frame, from one side to the other, using for this purpose boards with pointed ends, and digging away the earth from in front of each board as it was pushed in. Before beginning this, two long beams were laid lengthwise along the sides just below the frame, and the floor boards were so placed as to lie just above these, and rest across them. The entire structure, frame and floor, with the enclosed grave, thus came to rest upon the two beams, and when these were lifted by some six men, the whole was readily loaded upon an auto-truck.

In this form the aboriginal grave, weighing about six hundred to seven hundred pounds, was brought to the anthropological laboratory of Smith College, where the actual excavation took place at leisure, and under the most favorable conditions possible. The tools employed for the rougher work were wooden skewers, obtained at the market; also knives and scalpels could be employed when the act to be accomplished was definitely known, and where the bones were exactly located. The use of several sorts of brushes, especially the ordinary type of clothes brush, was of great assistance, and was fairly safe.

This work stretched over the available time of several days, and upon its completion the exposed surface was thoroughly impregnated with shellac, used very thin at first, followed by stronger and stronger solutions, which eventually bound everything in place, earth, bones and all. In order to search for artifacts the earth outside of the boundary of the bones was carefully removed, but was later replaced, mixed with cement, and formed a natural frame about the skeleton, which thus came to lie in a depression, as at first. The specimen has been placed in an oak case under plate glass, and is oriented as nearly as possible in the position relative to the points of the compass that it was when found.

While still lying in its crude box, but thoroughly excavated, the skeleton was photographed from directly above by means of an improvised staging, and the results are here given (fig. 56). To get the proper effect the photograph should be held horizontally with the eye of the observer directly above its center. The skeleton is lying upon its right side, with knees drawn up to the chest, and with the lower legs strongly flexed, and evidently resting on the hams when in the flesh. The arms are also flexed, with elbows touching the thighs; and the hands, placed closely together but not clasped, are close to the face.

In the original orientation the axis of the trunk, or of the entire body as a whole, lay north and south, with the head towards the south, while the face was turned directly towards the east, as in the case of the skeleton figured in my former article.[1] A comparison of the two cases shows a close similarity in the position, even to details.

If this may be taken as the customary position of the body in local aboriginal interments, in the case of males at least, and this seems extremely probable, some explanation should be advanced to account for it. The embryonic theory, that is, that the position of the *fetus in utero* is symbolic of the rebirth, although cited as probable in the previous paper of the senior author, has been rendered untenable through the searching criticism of Andrée.[2] The first mention of the folded body as the embryo was by Peter Kolbens in 1745 who used the comparison simply as a simile, without the slightest intention of suggesting any symbolism on the part of the Hottentots, of whom he spoke. This mention, wrongly interpreted as an explanation rather than as a figure of speech, started the theory, which seems to have nothing in folklore or primitive philosophy to confirm it, save the general belief in the earth as the All-Mother.

A careful study of this absolutely reliable specimen interment strongly suggests that the body when buried was securely tied up

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[1] *Ibid.*, pl. XXIII, fig. 2.

[2] Andrée, Richard; "Ethnologische Betrachtungen über Hockerbestattung," *Archiv für Anthropologie* (N. F.), Bd. VI (1907), pp. 282–307.

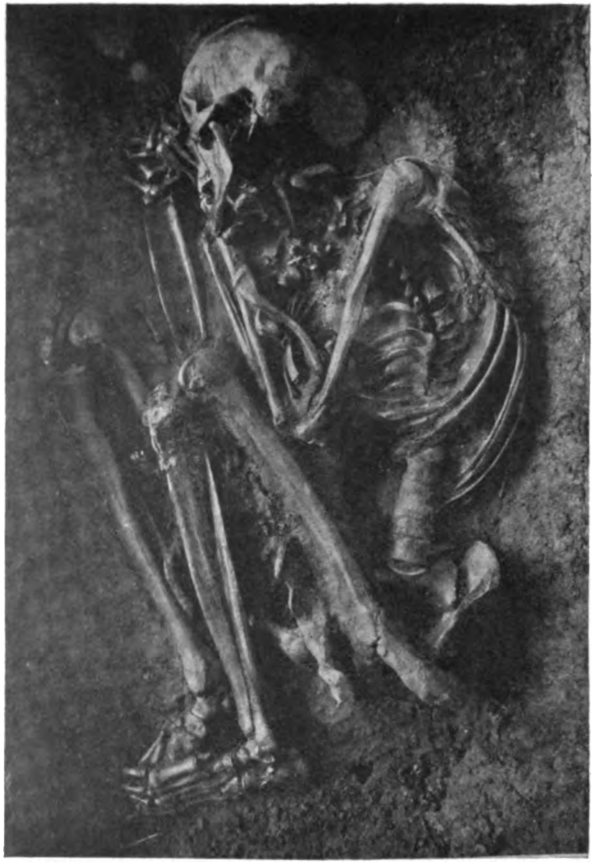

FIG. 56.—Aboriginal skeleton excavated at North Hadley, Mass., in November, 1915. The bones are exactly as found, as they have never been disturbed. Now in the Zoological Museum of Smith College. The photograph was taken from directly above the specimen, and should be held horizontally to put it into the proper position.

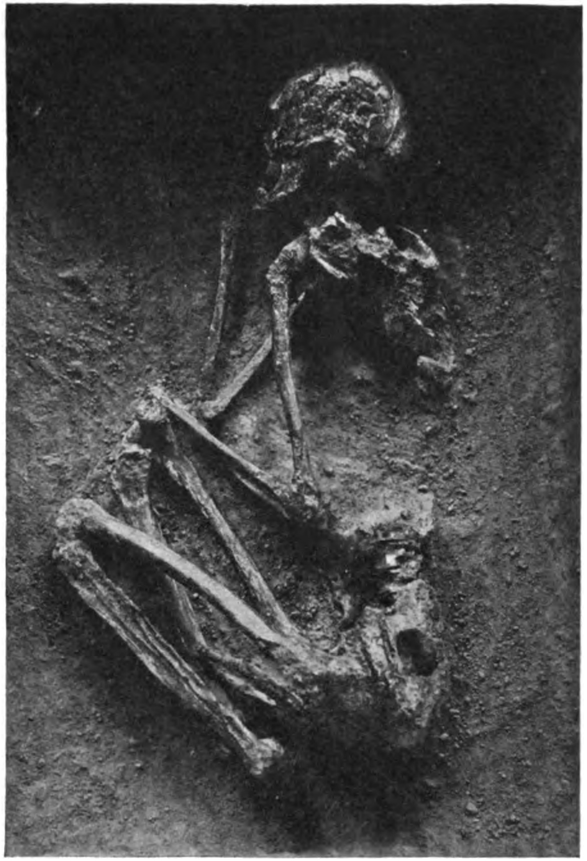

FIG. 57.—Aboriginal skeleton (Pocumtuck tribe), excavated at Greenfield, Mass., November, 1916. Condition and manner of photographing the same as in figure 56. Now in the Gilbert Indian Museum of Amherst College.

by what must have been heavy straps, probably of leather, as otherwise it would be hard to see how a dead body could be forced into such a position. Furthermore, while the most of the bones are either in their exact position or have been simply dropped through the natural decay of the body, the cervical vertebrae, and with them the clavicles and sternum, have been much dislocated, a condition perfectly explicable by assuming the presence of a tight strap, passed under the knees, and over the nape, forcing the head and neck down into a definite stoop. The gradual release of the joints of the body through decay, allowing the free parts to follow more the tension of the ligaments, while still confining the neck vertebrae, would produce exactly the result found in this case, and indicated with some definiteness in the photograph.

The fact that marked disarrangement of bones is shown mainly in this region is made especially significant from the study of other parts of the skeleton, where are shown only the definite results of the effect of decay and the influence of gravitation, and these clearly and vividly. Along the thorax, for example, when decay softened the ligaments which held the ribs to the vertebral column, this latter part rolled somewhat outward, parting from the heads of the ribs, which rested firmly in the earth, and had no tendency to move. This brought the vertebrae of this region up until the ventral aspect of the centers looks almost directly upwards, while the ribs of the upper side, the left ones, have dropped downwards in a bunch. The dropping down of the left knee, and the slight rolling over of the left foot, every phalanx of which has been preserved, are the natural results of gradual decay in place, while in contrast to these the strong dislocation of the vertebrae and other bones in the region presumably bound down by the thong is very striking.

There are two noticeable breaks, aside from that of the left tibia, previously spoken of, which need explanation, and these are (1) the fracture of the left femur near the proximal end, and (2) that of the left humerus, near the elbow. These are evidently comparatively late injuries, and are easily accounted for by the facts that this skeleton lay directly in the driveway which formed the only convenient means of reaching the field with a wagon, and over which several generations of Hadley farmers have passed up and down with their loaded carts, and that the burial, as is customary, was superficial, the highest parts being not more than twelve to fifteen inches below the surface. In fact, on the day of the find we drove a heavy auto directly over the skeleton, as it afterwards proved, no less than three times, which may well account for a fresh extension of the fracture of the thigh, noticeable when the place was thoroughly uncovered and made clean.

Thus, from the impossibility of putting a dead body into such a position without confining the limbs in some way; and from the testimony of the disassociated neck vertebrae, while elsewhere the normal dropping down of the parts from unhampered decay is plainly seen, it seems safe to conclude that this body was tied up, and that very tightly, when buried. A strong band was evidently passed beneath the knees and over the neck, and probably also the hands were tied at the wrists and the feet at the ankles.

We may get corroboration of this theory, that the corpse was tied up tightly before burial, as well as a very reasonable suggestion as to the purpose, from the paper of Richard Andrée[1] who makes an exhaustive review of the custom of "Hockerbestattung" in all countries, and among peoples of every age, so far as known. He sets aside the embryonal theory, previously touched upon, as too fantastic and improbable to be considered; he treats similarly the idea of this position as that assumed by primitive man in sitting or lying; and he finds little evidence that the folding up is due merely to the desire to get the body in as small a hole as possible, and thus save work for the grave-diggers. He refers to the almost universal idea of the return of the dead to alarm or torment the living, and to the extraordinary means often resorted to to prevent this, and finds here the true explanation of the bound corpse. This suggestion appears to have been first expressed by Schoetensack in the case of the Australians,[2] to which Andrée adds numerous other cases from all parts of the world, and at various periods of time.

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[1] *Archiv für Anthropologie*, Bd. VI (1907), pp. 282–307.

[2] *Verhandlungen der Berliner Gesellschaft für Anthropologie* (1901), p. 522.

Thus Strabo reports of the Troglodytes that they bound the neck of the corpse to the legs with withes of thorn, while remains of cords are still to be found in Egyptian graves of the Stone Age. In the island of Ceram, and also in Greenland the people are so anxious to tie up the corpse that they are led to begin operations before death. The same idea, that of not returning to haunt the living, appears in the funeral ritual of many peoples, both with and without the actual binding, and usually takes the form of direct appeal to the body not to come back.

Among some tribes more violent methods than simply tying are resorted to in order to prevent the return; and we find such customs as the smashing of the vertebral column with a heavy stone, as is done in Damaraland, or the burial at the crossroads with a stake of holly through the heart, as has been actually done in recent times in Europe.

In the article of Andrée here referred to is found a rich collection of burial customs in connection with the folding up of bodies for interment; and in all, at least wherever there is any special care used in the position of the body, there runs the thought of so fettering the body in the grave as to prevent a return. This fear may be founded on the idea that the body may avenge its death upon the living; that he may take back with him someone of the family still alive; or perhaps in certain cases, that a demon may get possession of the body and thus be enabled to do grave mischief, of a sort that, in his usual disembodied state, he would be powerless to effect.

The next occasion for obtaining local aboriginal skeletons came to the authors a year after the previous excavation, when, on November 22, 1916, some workmen on the street railway in Greenfield, putting in a spur track beside the power house at the locality known as Cheapside (fig. 58), dug through no less than three aboriginal skeletons, placed in almost an exact row, some twenty to thirty feet apart. But few traces of bones were left when we visited the spot, but there was the usual story about their being "in a sitting position," and one of the workmen went so far as to place himself in the exact position of one of the skeletons as he had observed it.

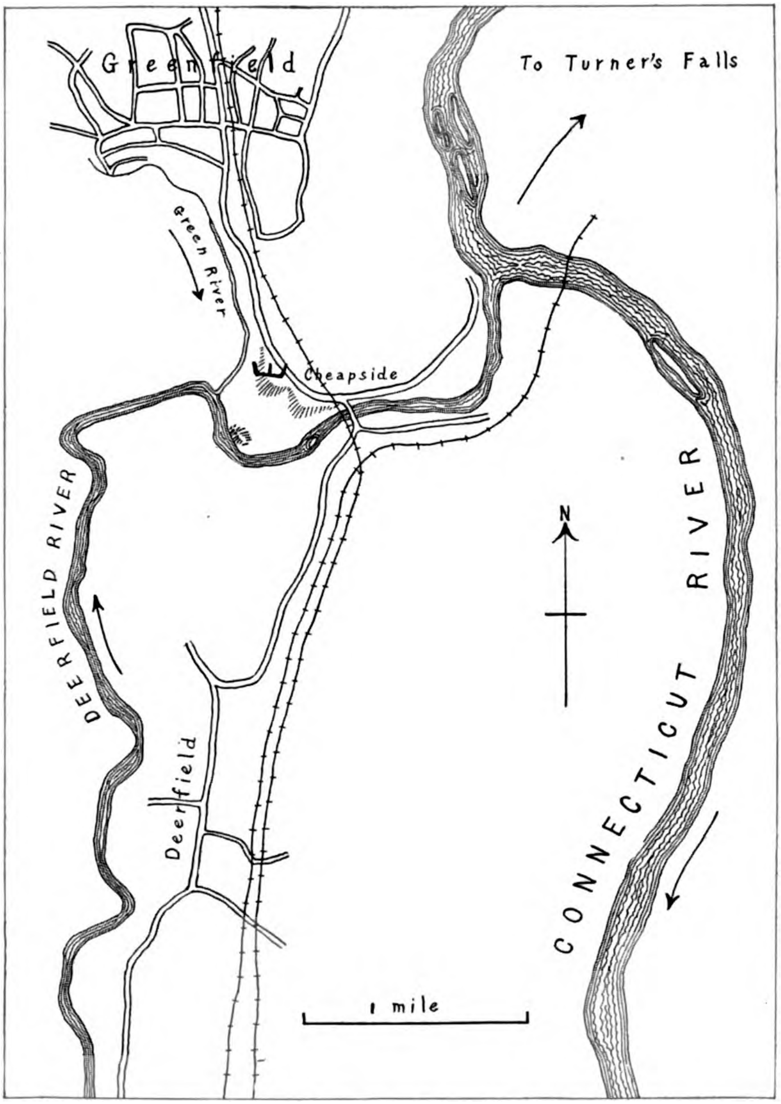

FIG. 58.—Sketch map, showing the locality where the second of the skeletons described in this paper was found. The first occurred at the site given in this periodical, vol. 5, p. 296.

Such could not, however, have been the case, since the entire excavation, from the surface to the level at which the workmen stopped, was but twenty-eight inches, while the mark of one skull was still visible in the frozen soil, the top ten inches below the surface. The stratum which included the entire skeleton could not therefore have been more than eighteen inches thick, quite insufficient to have included a sitting skeleton in the upright position claimed. Moved by this inexorable logic the workman next claimed that the leg bones were still in the earth below the previous excavation, but not only did further careful digging fail to reveal any bones, but did show undisturbed soil, while pieces of both femur and tibia were discovered among the debris left from the first digging. This refutation of the popular claim universally heard locally, that the Indians buried their dead in a sitting position, is extremely important, since, in the two cases of 1905, and the two recorded here, such was certainly not the case. They were sitting, in the sense that they were folded up, with the legs flexed, but were then placed on the right side, and were better described as lying, with the limbs drawn together. In the extremely careful description of Indian burials at Deerfield, the local historian, George Sheldon, describes all the graves the excavation of which he personally witnessed. The skeletons were lying in the flexed position, as in all of our cases, but in all instances cited from the testimony of others, the offending phrase is constantly used. It seems probable from the descriptions that in one or two instances of pretentious interments, where there was a rude stone cist for the reception of the body, the skeletons were really found sitting erect, with the skull on top, but until such a grave is excavated by an experienced archaeologist, and recorded by the aid of photographs, or, better yet, by the permanent preservation of the entire interment, some doubt may be allowed even here. Naturally our failure in finding sitting skeletons in this region does not disprove their presence here, and has nothing to do with interments elsewhere, but thus far, in four cases carefully excavated by ourselves, and in the case of some half-dozen others found by workmen, the skeletons were not sitting, but lying folded up, a position that could be easily mistaken for a sitting one if the grave were carelessly exhumed.

The place where these Greenfield skeletons were dug up was an ideal site for an Indian cemetery. The entrance of the Green river into the beginning of a U-shaped bend of the Deerfield encloses on three sides a piece of meadow several acres in extent. Along the fourth side runs a slight elevation in the form of a ridge, lying across the meadow, and forming the fourth side of the enclosure. This ridge is rather indefinitely divided into knolls, the westernmost being oval, and semi-distinct, measuring some 30 × 50 feet in diameter. This is the spot where all the skeletons have been found, as it is the only place where any digging has been done about here and of this perhaps two-thirds has already been leveled, resulting in the discovery of about a half-dozen graves. The remainder of the ridge, and a bit of raised ground at the other end of the meadow, almost on the river bank, seem likely spots for further excavation.

So far as can be ascertained there is no tradition connecting Indian activity with this particular knoll, but it lies in the center of activity of the once powerful Pocumtuck, who were the overlords of all the river tribes, Squakheag, Nonotuck, Agawam, etc., between Brattleboro and Hartford, and who in the middle of the seventeenth century, were rated by Gookin at about the same number as the Narragansett. The Dutch in 1652 classed them among the "Great Indians." During the earliest part of the period of English settlement they were at war more than once with Uncas, that questionable protégé of the English, and were in temporary alliance with the Narragansett, and Niantic, to aid them against their Indian foes. In 1664, after treacherously murdering two ambassadors from the Mohawk, who had come to make with them a league of lasting peace, they were attacked by the entire manhood of the offended Mohawk nation and literally wiped out. After this the Pocumtuck disappear from history, but a few wandering individuals of the tribe appear in the English records, generally in those of the court. One old woman, Mashalisk, who owned land on the east side of the river, and who probably owed her preservation to this location, appears as a seller of these lands in payment for the debts of her son, contracted at Pynchon's trading store in Springfield.

This definite date for the destruction of the Pocumtuck tribe allows us to date the use of this knoll as a cemetery as prior to 1664, and, from the fragile condition of the bones, far more so than in the case of the North Hadley site, it may well be one or more centuries before this time.

The fact of the number of skeletons discovered in removing a part of this knoll showed the strong probability that others are still present in the remaining part, and it is in this connection that we record with great pleasure the great courtesy with which our desire to dig here was met by the superintendent of the street railroad, Mr. John A. Taggart, who not only gave us full permission to excavate as we wished, but on various occasions sent men to assist in certain parts of the work which required aid, especially giving the services of the carpenter of the company, a man of great ingenuity and resourcefulness, who accomplished the boxing up of the grave in a most expeditious manner.[1]

Beginning the excavations by trenches mainly parallel to the excavated edge, we located a skeleton on the second day of work, finding first the upper end of a left femur, and then the associated tibia and fibula. Ascertaining the general position of this leg, we next located the skull, and the left humerus, after which, as in the other case, we surrounded the entire grave with a trench. As an improvement in the technique may be mentioned the fact that the frame that was to serve as the sides of the box was first made apart from the site, and strongly reinforced, then brought to the spot and pushed down to place, and that then the bottom boards were driven through from one side to the other by repeated gentle strokes of a heavy sledge hammer. This succession of light jars, contrary to what might be expected, caused less breaking or cracking of the earth than did the steady push of the crowbar used in the first case. Two planks were then placed beneath the whole, across the boards forming the bottom, and the whole was lifted to the surface by eight men. Here it may be said that it is of first importance to have the entire skeleton re-covered by earth, packed down firmly, before the work of boxing or lifting is attempted, and it is equally important to have all the lifting done by a sufficient number of men trained for this sort of thing to avoid any possibility of shock caused by slipping or jerking.

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[1] While this article was in press we have had occasion to conduct further excavations at this site, and have found three more skeletons. These were in the same position as the one described, and in all are seen the two differences from the position at North Hadley; (1) the legs not so much flexed, and with the femora extended out at nearly a right angle from the body axis, and (2) the erect position of the vertebral column. They were also oriented as the other, with the body axis north and south, and heads to the south, and when located on a plot with the position of the others, so far as could be learned, were seen to run in definite rows placed north and south, the bodies placed lengthwise in the rows, and not across them, as in our cemeteries.

As in the other case, the transportation to the laboratory was accomplished by means of an auto truck, and the subsequent excavation was performed in a similar manner. Here, however, the problem was a more difficult one, as the bones were much less well preserved, and even the strokes of a clothes brush brought away the surface in the form of fine flakes and removed the inner tissues. It was also much less complete, as the bones of the hands and of one foot were entirely gone, leaving no trace even, while of the other foot, the right, there remained only the calcaneum and talus. Here there was no question of lack of care in the excavation, for the work was continued slowly through several weeks, allowing the whole to dry out thoroughly so that much of the earth was removed by blowing. This phenomenon, frequently noticed in old skeletons, is the more remarkable since the distal ends of radius and tibia, adjacent to the hand and foot, are distinct and practically entire; while there immediately ensues nothing but the sand. The skeleton is also entirely gone through the middle of the trunk, the first few thoracic vertebrae being in place and well preserved, while they leave off suddenly, together with their ribs, not to be resumed until the last of the lumbar series is reached, when the bones again appear in place and practically entire. The presence of two isolated ribs located under the left femur, suggests a solution of this serious loss, and gives a picture of some burrowing animal pushing downwards and forwards relative to the skeleton, dislocating the two ribs as found, and either taking out the missing parts, or so opening up the soil that they soon disappeared through decay. The activity of a carnivorous animal, like a fox, while the body was still fresh is possible, but the space between the leg bones, in the present state at any rate, is hardly sufficient to allow so large an animal to pass far enough to deposit the ribs.

Finally, as to the position of the body, important similarities and important differences are shown in comparison with the other subject of this paper. The general position and orientation, folded limbs, body placed on the right side, and face towards the east, is the same in both, but the parts are not bound so tightly together. The back is straight, without the pulling down of the head and neck so conspicuously seen in the other specimen. The legs and arms, too, are not pulled up as close to the body; the legs are spread somewhat apart, and the left knee is not dropped down to touch the other leg. One hand only, the right, was held before the face; the other lay on the knee of its own side.

In hardening the bones and surface earth in this specimen a new fixing substance was suggested by the junior author, and used here with excellent results, namely, a thin solution of water glass. This formed an especially good coating over the soft and fragmentary surfaces presented by the bones, holding the parts in place, even when almost pulverized, as on the side of the skull. The subject was an old man, with senile jaw and a single tooth, visible in the photograph (fig. 57). It has every appearance of having antedated the one from North Hadley, by a great many years, as both were in similar soil, well drained.

In the above account we have been careful to give the details of our procedures, thinking that they may prove useful to others in other excavations. Since our own experience has been to find better and better ways of doing things with each venture, many improvements of our methods will suggest themselves to anyone who tries an excavation of skeletons *in toto*. Some application of sheet iron or similar thin material for running beneath the grave and forming the bottom of the box will undoubtedly be developed; also materials better than either shellac or water glass may be found for hardening the whole mass, and converting it into a thin slab more easily handled than a box of dirt. This latter is especially to be desired since present methods do not allow any examination of the soil immediately beneath the body, one of the likely places for associated artifacts.

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RESTORATION OF A CLIFF-DWELLER

By HARRIS HAWTHORNE WILDER

THE accompanying photograph (fig. 59) gives the result of a plastic reconstruction of a skull, by the method given in this magazine some five years ago,[1] but with the addition of the neck and shoulders. These additional parts are built directly on the bones of the same individual, which are fortunately in our possession, and therefore the proportions, although individual, cannot fail to be approximately correct. Their method of restoration differs widely from that of the skull, for while in this latter the soft parts are thin and to a considerable extent rigid and unvarying, we have here to deal with a movable skeleton, covered with muscles, both of which elements vary in relation and actual contour with each movement. It is thus necessary to first place the bones in the desired position, making allowance for capsules and other soft parts at the joints, and then build up the muscles, one at a time, using the muscle scars, processes, and so on as an indication of their position. Interspaces usually found filled with fat may be then filled in with the plastic substance, here plastilina, and finally a fairly uniform coating to represent the skin should be covered over the whole. Although I have never used it, it would seem advantageous to reconstruct the subcutaneous part of some material like clay,[2] which would harden when the work is done, and over which the outer layer could be modelled with more precision.

In the present case the considerable labor of properly adjusting the bones was obviated, since the skeletal parts had dried in their natural position, and had thus never been disassociated. This circumstance saved much trouble, and naturally furnished results that were far more reliable.

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[1] *American Anthropologist* (N. S.), vol. 14 (July–Sept., 1912), pp. 415–436.

[2] Permodello, put out by the Prang Co., would probably be very suitable for this work, as it remains soft so long as it is kept moist, but hardens when dry.

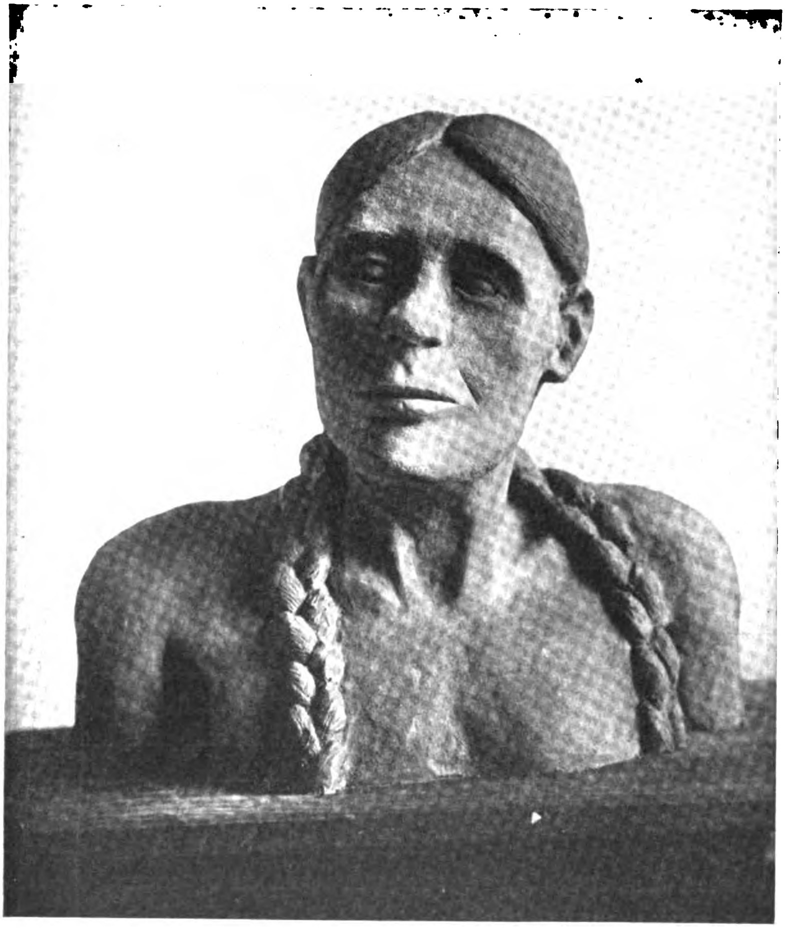

Fig. 59.

This method of restoring the soft parts from the muscle scars is not a new one, but has been employed more or less by the paleontologists, notably by Professor Lull in restoring the "frill" about the neck of *Triceratops*. His restoration of the Spy Man,[1] also, naturally took the muscles somewhat into consideration, although it was not built up directly on the bones themselves, as in the present case.

There are certain rather glaring errors in this restoration, due largely to inexperience, which could readily be corrected in another case; the most noticeable being, perhaps, the too high arm pit, the result of a wrong estimation of the breadth of the pectoralis major, as it runs out towards its insertion. The shoulders seem rather too high, but this is probably an individual peculiarity, as it was given by the bones in their natural position. There may be noticed in the photograph a curious twist of the end of the nose, which is apparent rather than real, and due mainly to the way the shadow falls. A careful examination of the light side of this organ shows that it is built out as fully as on the other side, and that, although in fact slightly scoliotic, there is not that drawing up of the right side that appears here.

The special matter of interest, however, and the main reason for the presentation of this paper, is that the woman here restored is the same one whom I figured in this journal thirteen years ago, as restored from the condition of a sun-dried mummy by the use of caustic potash, and it is thus of interest to compare the photograph here presented with those shown in Plate II of volume 6 (Jan.–March, 1904). Naturally there is no especial similarity in the *expression* in the two preparations, since in the former case the lips were wide apart, one eyeball was replaced by cotton, the nose had not been brought out to its full extent, and there was no hair on the head. If one recall the ease with which a good actor may become unrecognizable by simply distorting his face, he can see how fugacious and changeable a thing facial expression is. The main *features*, however, are not very different in the two cases, and the recent restoration may well represent the former one, brought to life. In the restored mummy one saw a young middle-aged woman, rather thin than stout, with high shoulders and possibly a slight stoop; who had borne children, and been employed during her lifetime in the hard, active work of aboriginal existence. This sort of person is shown by the new plastic restoration, and, while I naturally had all this in mind while making the restoration, the definite restrictions of anatomical relations and of the measurement of thicknesses were absolutely adhered to, and no unauthorized touch was allowed, save the lines about the mouth and the slight furrowing of the forehead, which were probable, though not definitely indicated.

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[1] *American Journal of Science* (Feb., 1910).

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IROQUOIAN CLANS AND PHRATRIES

By C. M. BARBEAU

THE Iroquoian race, formerly occupying most of the territory extending from the Great Lakes to the present states of Carolina, consisted of the Cherokee, the Tuscarora, the Five Nations or Iroquois proper, the Huron-Wyandot, the Neutral, and the Erie, with a total population aggregating from eighty thousand to one hundred thousand at the time of the discovery. Each one of these peoples was made up of many smaller nations, divided into tribes, and the tribes into towns and villages with an average population of less than two thousand souls. While political independence was, as a whole, the essential attribute of the village or town, there existed a marked tendency towards wider centralization which, in some cases, resulted in the federation of towns, tribes, and even nations.

Of far greater importance was the Iroquoian kinship organization, the ramifications of which extended far beyond the national frontiers. The only fundamental kinship unit, universal among the Iroquoian and neighboring peoples, is the totemic clan. Other relationship organizations, such as the phratry and the family, were either sporadic or subsidiary. The family, for one, was an ill-defined and amorphous group of close relatives tracing their descent to the same *remembered* and immediate maternal ancestor. There was no marked distinction between different families within the clan, which were supposedly issued from a common remote maternal ancestor. The clan was to the families what the tree is to the limbs. The more interesting relation between the phratry and the clan is the main object of the present study.

The Iroquoian and neighboring Algonkian[1] clans, on the whole, were widely diffused exogamic kinship units, endowed with in-conspicuous crest and supernatural guardian systems, the membership of which was inherited in the maternal line or conferred through adoption. As will be seen in the following lists, the clans were few in number; but their component parts were found disseminated in sections of numerous towns of many tribes and nations. Distance and linguistic barriers did not prevent the members of the same clan from acknowledging their relationship and mutually abiding by the rules of exogamy and fraternal help.

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[1] Under the name "Algonkian" are included the nations speaking an Algonkian dialect.

Of the Cherokee clans little is known besides the fact that they once numbered fourteen, and are now supposed to be "seven clans"—a reference to a mystic number. "The Wolf clan was the largest and most important" (J. Mooney); the Deer, Bird, and Paint clans are the only other ones the English names of which are given.

The number of the Tuscarora clans is not definitely known. Hewitt gives four slightly different lists, in which the order varies, presumably because they referred to different parts of the nation. The clans are: (1) the Bear, which appears the first in every list; (2) the Wolf, divided in one place into Gray Wolf and Yellow Wolf, which is given as second, fifth, and seventh in different lists; (3) the Turtle, twice given as subdivided into Small Turtle and Large Turtle, appearing in the third and fourth place; (4) the Deer clan, to which the third rank is granted in two lists, and the fifth and sixth rank in the two others; (5) the Eel clan, appearing in the second place in three lists, and sixth in the other; (6) the Beaver clan, to which the fourth, fifth, or seventh place is granted; and (7) the Snipe clan, given everywhere as the last. Morgan, on the other hand, states that, when the Tuscarora were admitted, about 1712, into the Iroquois League, their eight "gentes" were arranged into two sides or phratries, as follows:—first phratry: (1) Bear, (2) Beaver, (3) Great Turtle, (4) Eel; second phratry: (1) Gray Wolf, (2) Yellow Wolf, (3) Little Turtle, (4) Snipe. The Deer, he adds, became extinct in modern times.

There was no uniformity in the arrangement of the clans among the Iroquois proper or Five Nations. The Mohawk and the Oneida, the easternmost nations, seem to have had only three independent clans each, which are given in the following order: (Morgan) the Bear, the Wolf, and the Turtle; (Goldenweiser) the Turtle, the Wolf, and the Bear. On one modern Iroquois reserve, Goldenweiser has found these clans subdivided into three component parts of the same nature. The clans of the Onondaga nation belonged to two phratries. The first phratry consisted of the Wolf, Turtle, Snipe, Beaver, and Ball clans; the second, of the Deer, the Eel, and the Bear (Morgan). The first of the Cayuga phratries claimed the Bear, Wolf, Turtle, Snipe, and Eel clans; the second, the Deer, Beaver, and Hawk clans (Morgan). The Seneca nation, the westernmost one, consisted of the Bear, Wolf, Beaver, and Turtle clans, in the first phratry; and the Deer, Snipe, Heron, and Hawk, in the second (Morgan); or, according to Goldenweiser, of the Turtle, Bear, Wolf, and Ball, in the first phratry; and the Hawk, Deer, Snipe, Duck, and Eel, in the second. The Deer, however, is not found in the modern list.

In a manuscript dated 1666,[1] it is stated that the Iroquoian nation was divided into two bands, respectively of four and five families. The first band was named Gueyniotiteshesgué, which is said to mean four families; and the second, Ouichiniotiteshesgué, *i. e.*, five families. These families (clans) are given as follows: in the first band, 1. the Turtle, 2. the Wolf, 3. the Bear, 4. the Beaver; in the second, 1. the Deer, 2. the *Grand Pluvier* (large plover), 3. the Wild Potatoe, 4. the *Petit Pluvier* (small plover), and 5. the *Kilion* (a bird, possibly the Hawk).

The Wyandot clans seem, at least theoretically, once to have been grouped into two phratries and a third odd unit, the Wolf clan. One phratry consisted of the Big Turtle, Mud (small) Turtle, Prairie Turtle, and Hawk clans; and the other, of the Deer, Bear, Porcupine, and Beaver clans. The Snake and the Snipe clans, moreover, have, in modern times, been added to that list. Informants state that the Snake clan originated out of the Deer clan, with which it is still related. As to the Snipe clan, it was recently introduced from the Cayuga-Seneca system into that of the western

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[1] Dominion Archives of Canada, Ottawa, in the *Correspondance Générale*, F 2 (1663–1667), p. 2.

Wyandot. The Lorette Huron claim to have belonged to four clans, the Turtle, the Bear, the Deer, and the Wolf. There is no need of giving here earlier lists, such as those of Finley, Clarke, Walker, Morgan, Powell, and Connelley, which, on the whole, agree with that of the author given above. Some confusion, due to the lack of a careful study, is to be noticed in the case of the many Turtle clans, which, according to Connelley, would be as many as five. The so-called Eagle clan, in Finley, is evidently a misnomer for the Hawk clan. Nothing is known of the clans of the extinct Erie and Neutral nations.

In the various Algonkian, Siouan, Muskhogean, Yuchi, Pueblo, and other lists of clans we find, unevenly represented, most of the Iroquoian clans, besides many others.[1] The diffusion of the same clans here is in all probability due to direct transmission or to imitation.

The Wolf clan seems the most important and widely distributed among the Algonkian, the Iroquoian, the Siouan, and other eastern, southern, and central nations. It is found among the Cherokee, Tuscarora (Gray and Yellow Wolf), Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot, Huron, Mohican, Abenaki, Ojibwa, Potawatomi, Miami, Menomini, Shawnee, Sauk and Fox, Kickapoo, Oto, Missouri, Winnebago, Mandan, Chickasaw, Creek, Yuchi, Hopi, Taos, and presumably many others.

The Bear clan is found among the Tuscarora, Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot, Huron, Mohican, Abenaki, Ojibwa, Potawatomi, Miami, Menomini, Shawnee, Sauk and Fox, Kickapoo, Iowa, Oto, Missouri, Winnebago, Quapaw, Ponca, Kansa, Omaha, Pawnee (?), Creek, Yuchi, Acoma, Hopi, Zuñi, Sia, Taos, Walpi, Nambe, and Pecos.

While the Deer clan is not found among the easternmost Algonkian, it obtains among the Cherokee, Tuscarora, Onondaga, Cayuga, Seneca, Wyandot, Huron, Shawnee, Menomini, Winnebago, Quapaw, Kansa, Omaha, Pawnee (?), Chickasaw, Creek, Yuchi, Hopi, Zuñi, Sia, and Pecos.

The Turtle clan, on the other hand, has a more easterly distribution than the Deer, namely among the Tuscarora (Small and Large Turtles), Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot (Big, Mud, and Prairie Turtles), Huron, Delaware, Munsee, Mohican (Little, Mud, and Great Turtles), Ojibwa (Mud, Little, and Snapping Turtles), Menomini (Mud Turtle), Shawnee, Quapaw, Omaha, and Yuchi.

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[1] Morgan, *Ancient Society*.

The distribution of the following clans is limited to fewer nations. The Beaver clan is found among the Tuscarora, Onondaga, Cayuga, Seneca, Wyandot, Huron, Ojibwa, Abenaki, Potawatomi, Menomini, Oto, Iowa, Pawnee (?), Yuchi, Creek, and Quapaw nations. The snipe clan is found among the Tuscarora, Onondaga, Cayuga, Seneca, Wyandot (recently introduced), Ojibwa, Sauk, and probably many others. The Eel clan has its constituent members among the Tuscarora, Onondaga, Cayuga, Seneca (according to Goldenweiser), and Mohican (Yellow Eel). The Hawk clan is found among the Cayuga, Seneca, Wyandot, Abenaki (Pigeon Hawk), Ojibwa (Pigeon Hawk), Potawatomi (Black Hawk), Menomini, Sauk and Fox, Hopi, and Sia. The Snake clan, recently introduced among the Wyandot, belongs to the Abenaki, Ojibwa (Snake and Water Snake), Shawnee, Iowa, Oto, Missouri, Winnebago, Quapaw, Ponca, Yuchi, Walpi, Pecos, Hopi, and Zuñi. The Porcupine clan is found among the Abenaki, Wyandot, Menomini, and Hopi. The Duck clan belongs to the Seneca (according to Goldenweiser), Ojibwa, Menomini, Hopi, and possibly some other central tribes. The Ball clan seems to belong exclusively to the Onondaga and (according to Goldenweiser) the Seneca. The Heron clan is found among the Seneca, possibly the Mohican, the Ojibwa, Menomini, and some Pueblo tribes. The Wild Potatoe clan, which a 1666 manuscript [1] gives as Iroquois, belongs also to the Sauk, Yuchi, and Creek.

The clans, among some Iroquoian and other nations, were federated into larger exogamous bodies, now termed phratries, the main function of which seems to have been political. The distribution of the phratries by itself indicates that they must have spread gradually and in comparatively recent times among the few Iroquoian and Algonkian nations who possess them, and that their growth is historically disconnected from that of the clans. Out of eight Iroquoian nations only four surely had a phratric system. These are the Tuscarora, the Onondaga, the Cayuga, and the Seneca. The Mohawk and Oneida clans may possibly have been subdivided into smaller clans, standing in the relation of clans to phratry; but this point has still to be proved. Among the Wyandot, moreover, it seems that once an attempt at least was made to arrange the clans into phratries.

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[1] Morgan, *ibid.*

Phratries were also found among some of the neighboring Algonkian nations, such as the Delaware, who had three phratries, as follows: I. Wolf, Bear, Dog, and Opossum; II. Little Turtle, Mud Turtle, Great Turtle, and Yellow Eel; III. Turkey, Crane, and Chicken clans.

The occurrence of the phratries and moieties, although sporadic, is fairly common in the North American tribes and among people whom the Iroquoian tribes are likely once to have come into direct or indirect contact with, such as the Yuchi, the Siouan tribes, the Muskhogean tribes, and the Pueblo and Mexican Indians. It is therefore safe to consider this institution as a derived rather than an original and exclusive one.

Since our subject-matter has a direct bearing on the often discussed history of the phratries and clans, we will now consider some characteristic features in the nature and growth of those of the Iroquoian tribes and their neighbors. L. H. Morgan, in 1877, explained the growth of phratries in the following words:[1]

> It proves conclusively the natural process by which, in course of time, a gens [clan] breaks up into several, and these remain united in a phratric organization, which is expressed by assuming a phratric name. . . . It shows . . . that the phratry is founded upon the kinship of the gentes.

This generally accepted hypothesis on the origin of the American phratries was also applied by A. Lang to some Australian phratries; and Goldenweiser has lately adhered to the same view, expressing his belief in the priority in time of the phratry over the clans among the Iroquoian tribes. This point has since been debated. It seems, however, that this hypothesis is untenable, as the clans, and not the phratries, are universal, ancient, and essential among the Iroquoian and neighboring nations.

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[1] L. H. Morgan, *Ancient Society*, 1907 ed., p. 100.

Our reason for considering the phratric and clan systems, in the Eastern Woodlands, as unrelated to each other in their development may be outlined under the following headings: I, The geographic distribution of the clans and the phratries in North America does not coincide; II, while clans are universal among the Iroquoian tribes and most of their neighbors, phratries appear only sporadically in a minority of Iroquoian nations, and seldom in their immediate vicinity; III, the lack of uniformity in the arrangement of clans in the phratries reveals an arbitrary purpose rather than unconscious growth and transmission; IV, while the clans have never ceased to the present time to be at the basis of the social system, the phratries have disappeared together with the Iroquoian confederacy; V, the totemic attributes of the clans were never shared by the phratries, with the exception of the accidental exogamic rule, which proved to be more persistently a clan than a phratric feature.

I. Eponymous clans seem to be the most typical and widely distributed form of kinship organization in North America. Phratries, on the other hand, cover a much smaller expanse, and their essential traits are not necessarily grafted upon clans or confined to their area of diffusion; but, among many western tribes, they appear in the form of tribal, exogamic, or ceremonial moieties.[1] There is little doubt that, in the course of time, the moiety or phratric system as well as specific eponymous clans spread independently in various directions, from as many unique, but not necessarily identical, centers. While the clans may claim a considerable antiquity in the eastern half of North America, the phratries and moieties are distinctly at home in the southeast, center, southwest, and Mexico, wherefrom the Iroquoian ones must have been derived. It is, moreover, remarkable that wherever the phratric system is wanting as a whole the clans show no tendency towards growing into phratries, in the manner above described.

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[1] The term moiety is here used to refer to an aggregation of non-totemic social units, whether such aggregations are two or more in number.

II. Practically none of the Iroquoian clans is exclusively its own. Wolf, Bear, Deer, and Turtle clans are not only found in every Iroquoian nation, but in a great many tribes from the Atlantic coast to Arizona. The identical three Turtle clans, on the other hand, obtain only among the Ojibwa, Wyandot and Delaware; and two are claimed by the Tuscarora. The obvious centers of diffusion of the Hawk, Snake, Porcupine, Duck and other minor clans lie outside of the Iroquoian frontiers, wherein they were introduced only at one or two places. It is still remembered among the Wyandot, for instance, how and about when the Snake and Snipe clans came into existence; the Snipe clan having branched off from that of the Seneca, and the Snake clan having appeared when they were living in close contact with the Ojibwa, who had a Snake clan.

While to be an Iroquoian Indian it was essential to belong to a clan, it may be said that the phratric system affected only a minority of their population, belonging possibly to less than one half of their nations—that is the Onondaga, Cayuga, Seneca, and Tuscarora. The Cherokee and Huron had no phratries, as far as we know. The three Mohawk and Oneida clans have been considered merely as such. And the actual or prolonged existence of Wyandot phratries is problematical. The Delaware phratries were a strikingly exceptional development among the Algonkians, and were evidently due to Iroquoian influence.

III. Had the most ancient clans of the Iroquoian tribes grown into phratries by gradually splitting up into related constituent clans still retaining their genetic connection, we should now find evidence of this ancient process. The reverse is true, however, as we have seen above.

The arrangement of the clans within the phratries, moreover, does not show any genetic development, as they are found now in one, now in an opposite phratry. The Wolf clan was the head of a phratry among the Onondaga, Tuscarora, and Delaware. It was a subdivision of the phratry headed by the Bear among the Cayuga and Seneca. In the above-mentioned manuscript list it is given as a subdivision of the group headed by the Turtle. And among the Wyandot and other nations it stood alone. The Bear clan was at the head of a phratry among the Tuscarora, Cayuga, and Seneca; but it came last in the Onondaga phratry under the leadership of the Deer, and second in the Wyandot phratry under the Deer clan and in the Seneca and Iroquois lists of Goldenweiser and the above-mentioned manuscript. It stood independently elsewhere. While the Turtle (or Big Turtle, when there is more than one) was the leader of a Delaware and an hypothetical Wyandot phratry and is also given that rank in the old Iroquois manuscript referred to, it is given after the Wolf and the Bear clans, in the same phratry, among the Tuscarora, Onondaga, Cayuga, and Seneca. The Deer clan, which is almost uniformly given as the head of a phratry among the Onondaga, Cayuga, Seneca, and Wyandot, does not seem to exist in the Mohawk, Oneida, and Tuscarora nations. Although the Hawk clan, when it is found, is of minor rank, it is given by Goldenweiser as the head of a Seneca phratry; and the Deer clan comes only after. The same thorough lack of consistent arrangement—if we consider phratric relationship—is found in the case of every clan, of whatever importance. Notwithstanding the probable fact that the three Turtle clans were derived from one another, two of their subdivisions are found in opposite phratries among the Tuscarora. The Bear and Deer clans, in three nations. are found in the same or corresponding phratry, and in three others in the opposite one. The Wolf clan is either associated with the Turtle clan or is independent. In the two neighboring Seneca and Cayuga nations the Beaver clan is found respectively in the phratry led by the Deer clan and in the opposite phratry. And so on.

If such a state of affairs could never be explained satisfactorily in the light of the contested hypothesis, it becomes logical enough if we assume that the guiding principle in the arrangement of clans into phratries was the deliberate consideration of the balance of power, or the approximately even division of numbers between the two or three phratries, in the national councils or in the ceremonial functions. In other words, it can only be understood through the assumption that a conscious political federation of clans once took place in several Iroquoian nations, together or severally; such a move being probably fostered by the imitation of a similar foreign example and by the desire of improving the national efficiency through a greater unity in its councils and wars.

IV. Although the few Iroquoian phratries probably existed before the creation of the League or Confederacy (about a century before the discovery), the clearness of their outlines must partly be due to their functional importance in the operation of the Confederacy, which they have followed in its downfall, thus allowing the clans once more to become exclusively supreme, as in their original condition. The only vestiges of the former existence of the phratries in the four Iroquoian nations that once possessed them are to be found in the memory of informants, in myths, and in the more palpable forms of many surviving rituals and ceremonies. The "brother and sister" relationship and connected exogamy once embracing all the clans in the same phratry have long been discarded or forgotten; and, in the course of a statistical investigation on this point, Goldenweiser could only notice a weak tendency among the older people towards avoiding intermarriage between the clans formerly of the same phratry. The only informant possessing the Wyandot tradition about the phratries strongly denied that exogamy ever applied to the phratries, although his recited text plainly implied it. The same text, moreover, describes the creation of the Wyandot phratries as having taken place in a great national council at which several foreign tribes attended.

It should also be remembered that, with the exception of the exogamic taboo, the phratries were operative factors only in the case of a reunion of a national council or of important ceremonials. The respective centers of the various clans were usually scattered far apart, in different towns; and these had to be convoked and represented in order to fulfil their most essential duty. In the absence of councils or ceremonials, the phratries were only latent, and a continued inactivity meant their disappearance. This in itself indicates the nature of their elaborate political origin or federation of naturally dispersed elements.

v. The Iroquoian clans are typically totemic; but this cannot be said of the phratries. While the heraldic and supernatural aspects of Iroquoian totemism were moderately developed in the clans, the phratries did not share these traits in the slightest degree. The clans were eponymous, or named after a certain animal designated in a metaphorical term; but the phratries were nameless, although ethnographers have a tendency, for the sake of convenience, to name them after their head clan. In the 1666 manuscript, as we have seen, the first phratry was termed "the-four-families," and the second "the-five-families," from the fact that there were four clans in one and five in the other. Each clan owned exclusively a list of individual names, a varying proportion of which referred to the clan animal; such a proportion being large among the Wyandot, and trifling—we are told—among the Iroquois proper. Although the clan animal was considered as a supernatural protector of his human confederates, the evidence here is rather scarce, and points to the fact that this totemic trait may originally have been still less marked than the ancient Iroquoian custom of representing pictorically the clan totem on various objects belonging to the clan or its members.[1] As there were no phratric, but only clan, animals, the phratries as such were devoid of all the totemic features which had not been extended to them by the clans. And exogamy, which —we assume—at first was a clan taboo, resumed its earlier form as soon as the phratries dropped out of existence. When citizenship was conferred upon a native child or a stranger it was only through his adoption into a clan; the phratry was not vitally concerned in this process.

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[1] The above-mentioned manuscript states that each family had the picture of its "family" animal painted in the gable of its house, some in black and others in red (p. 395).

PARALLEL BETWEEN THE NORTHWEST COAST AND IROQUOIAN CLANS AND PHRATRIES

By C. M. BARBEAU

THE following brief parallel between the tribal and kinship groups of the Northwest Coast tribes and those of the Iroquoian stock was first meant to accompany the above article, "Iroquoian Clans and Phratries" and a paper entitled "Growth and Federation in the Tsimshian Phratries," published separately in the *International Congress of Americanists*, Washington, 1915.

There is a close similarity only in the village or tribal system in both areas. Although the culture of these peoples was of a different type, their village communities were alike in so far as they were more or less permanent complexes of kinsmen and mutually unrelated people. In other words, they embraced members of different exogamous groups, either families, clans, or phratries. While there is no doubt that, in one or more villages, the members of one clan or phratry were predominant, it is now too late to map out precisely what were the former respective centers of influence of the many Iroquoian clans. Notwithstanding the fact that the village communal life had a certain tendency to create a bond of sympathy between its unrelated members, the kinship ties between relations living in different villages always remained the fundamental factor in their social organization.

When we compare the families, clans, and phratries of the Northwest Coast with those of the Eastern Woodlands, we find some comparable features, though much specialized in different directions, and many striking differences. Among the Tsimshian and neighboring nations of the coast, the phratries, clans, families, and even houses, were totemic units, and were patterned after one another just as if the smaller unit were an offshoot of the larger one. It was quite different among the Iroquoian tribes, where the clan alone was totemic. The degree of development of the various individual totemic features naturally differed in these two areas. The belief in the supernatural protection and guidance of the group totem was nowhere in the foreground, although present; but while the use of its pictorial or plastic representation as a crest was quite trivial among the Iroquoian tribes, it reached an abnormal development on the West Coast. The lists of individual names belonging exclusively to the clans among the Iroquoian tribes, and either to the phratries, the clans, the families, or the houses, on the Coast, reveal an uneven tendency to refer to some real or mythical traits of the totem; such references being more common on the West Coast. Exogamy was everywhere the main phratric attribute, although, among the Iroquoian tribes, it was derived from the more typical and persistent clan exogamy.

What strikes the observer as the most fundamental fact in the Iroquoian social system, taken as a whole, is the clan. On the Coast, however, the phratries and the families alone are plainly discernible. To get some knowledge of the clans one has to study the history of the people and unravel their complex relationship system. The clan here, in the course of time, gradually loses its individuality in the phratry. The Iroquoian clan, on the other hand, always retained its marked individuality within the phratry, and when one of the two disappeared it was the phratry and not the clan. Contrarily, the downfall of the ancient customs, on the West Coast, has left the phratries firmly established in the mind of the natives, while the frontiers between the clans now have almost disappeared. When, moreover, the neighbors of the Iroquoian tribes borrowed or imitated their social system, the clan was the usual model. But most of the interior neighbors of the Northwest Coast people have borrowed the familiar Coast phratries, presumably without duplicating their constituent elements.

The difference between the phratries in both areas is radical. The Coast phratries are totemic, the Iroquoian ones are non-totemic and purely political. The first grew around a clan nucleus, both through the differentiation of its parts and the numerous gradual outside accessions; the second were due to the arbitrary and sudden federation of hitherto unrelated clans into an evenly balanced political body. As a consequence, while the Iroquoian phratries were operative chiefly in tribal or national councils and feasts, those of the West Coast invaded every phase of domestic life.

In conclusion, we may mention the perplexing problem of the possible historical relation between the West Coast and the Eastern Woodland phratries through many intermediate stages. Although it is not in our power to find a solution of this question here, we feel that the various phratric and moiety systems, appearing sporadically in many parts of North America, may have had a common remote origin or a single center of diffusion.

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GROWTH OF THE NASAL BRIDGE IN CHILDREN

By LOUIS R. SULLIVAN

THE material used in this investigation was collected by Professor Boas in 1890 in connection with a series of measurements on school children of Worcester, Mass. The other measurements have been treated elsewhere.[1] The following report is in the nature of a laboratory exercise in a course on "Growth" at Columbia University under Professor Franz Boas who planned and supervised the work.

The shape of the nasal arch was recorded by means of a stout lead wire bent across the bridge of the nose from the inner canthus of one eye to that of the other. The cross-section thus obtained was then transferred to paper by tracing. In measuring a specially prepared transparent instrument marked off in millimeter squares was used. The transverse diameter of the cross-section of the bridge of the nose was measured at points 2, 4, 6, and 8 millimeters from the apex. The average for each year, the variability, and number of cases are recorded in Table 1.

TABLE I

TRANSVERSE DIAMETER OF NASAL BRIDGE AT POINTS 2, 4, 6, AND 8 MM. FROM APEX
(Measurements in mm. a = average for each year. σ = variability.
n = number of cases)

Boys

Age in Years	2 mm. from Apex			4 mm. from Apex			6 mm. from Apex			8 mm. from Apex		
	a	σ	n	a	σ	n	a	σ	n	a	σ	n
5	7.6	± .60	5	11.3	± .40	5	15.2	± .80	5	19.8	± .60	4
6	6.8	±1.03	30	10.1	±1.26	30	13.4	±1.91	30	16.8	±2.47	26
7	7.0	±1.01	51	10.1	±1.43	51	13.2	±1.96	51	16.1	±2.76	43
8	7.0	±1.21	45	10.3	±1.53	45	13.2	±2.15	45	16.3	±2.72	42
9	7.0	±1.45	55	10.1	±1.98	55	13.4	±2.66	55	16.5	±3.28	44
10	7.0	±1.00	49	10.2	±1.20	49	13.3	±1.82	49	16.5	±2.19	47
11[2]	—	—	—	—	—	—	—	—	—	—	—	—
12	7.2	±1.13	50	10.1	±1.70	50	13.0	±2.62	50	15.7	±3.61	44
13	6.9	±1.08	46	10.0	±1.52	46	12.7	±2.13	46	15.9	±2.83	44
14	6.8	±1.08	29	9.7	±1.65	29	12.1	±2.55	29	14.3	±3.21	28
15	6.5	±1.13	25	9.5	±1.23	25	12.2	±1.83	25	14.7	±2.59	25
16	6.7	± .97	10	8.9	±1.27	10	10.8	±1.47	10	12.6	±1.60	10

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[1] Boas, Franz, and Wissler, Clark, "Statistics of Growth," *Report of the Commissioner of Education for 1904.*

[2] 11 year old boys missing.

Girls

5	7.3	± .87	12	11.4	±1.29	12	15.3	±1.64	12	17.5	±1.11	4
6	7.2	± .90	20	11.0	±1.40	20	14.9	±2.23	20	16.8	±1.77	8
7	6.7	± .88	33	9.9	±1.34	33	13.0	±1.80	33	15.4	±2.40	19
8	6.7	±1.02	34	9.9	±1.37	34	12.9	±1.79	34	15.2	±2.21	21
9	6.7	± .82	34	10.0	±1.41	34	13.1	±2.12	34	15.5	±2.48	26
10	6.9	±1.01	35	10.4	±1.37	35	14.0	±2.14	35	15.2	±2.63	10
11	6.7	±1.02	40	10.1	±1.42	40	13.5	±2.61	40	15.9	±2.21	25
12	6.6	±1.06	46	9.5	±1.31	46	12.3	±1.94	46	14.3	±1.74	38
13	7.1	±1.22	31	10.2	±1.75	31	13.2	±2.18	31	15.9	±2.85	28
14	7.1	±1.15	37	10.0	±1.64	37	12.4	±1.80	37	14.8	±2.46	31
15	6.7	±1.25	16	9.4	±1.73	16	12.0	±2.37	16	13.5	±2.25	12
16	7.1	± .86	5	9.7	± .75	5	11.9	± .58	5	13.5	± .89	5

Inspection of this table shows a gradual decrease of the transverse diameters with increasing age. The irregularity of the decrease is undoubtedly due to the small number of cases recorded and to the inaccuracies inherent in the method adopted.

In order to obtain a clearer picture of the character of growth, we have assumed the transverse diameters to decrease proportionally to time, so that the diameter w_t at the age t may be expressed by the formula

$$w_t = a + bt.$$

This reduction gives the following results:

TABLE II

THEORETICAL VALUES FOR TRANSVERSAL DIAMETERS OF NASAL BRIDGE AT POINTS 2, 4, 6, AND 8 MM. FROM APEX

Girls					Boys				
Age	2 mm.	4 mm.	6 mm.	8 mm.	Age	2 mm.	4 mm.	6 mm.	8 mm.
5	6.9	10.6	14.2	16.8	5	7.1	10.5	13.6	17.1
6	6.9	10.5	14.0	16.5	6	7.1	10.4	13.5	16.9
7	6.9	10.4	13.8	16.2	7	7.0	10.3	13.4	16.7
8	6.9	10.3	13.6	16.0	8	7.0	10.2	13.3	16.5
9	6.9	10.2	13.4	15.7	9	7.0	10.2	13.2	16.3
10	6.9	10.1	13.2	15.4	10	7.0	10.1	13.1	16.1
11	6.9	10.0	13.0	15.2	11	7.0	10.0	13.0	15.9
12	6.9	9.9	12.8	14.9	12	7.0	10.0	12.9	15.7
13	6.9	9.8	12.7	14.6	13	7.0	9.9	12.8	15.5
14	6.9	9.7	12.5	14.3	14	7.0	9.8	12.7	15.3
15	6.9	9.7	12.3	14.1	15	7.0	9.8	12.6	15.1
16	6.9	9.6	12.1	13.8	16	7.0	9.7	12.5	14.9
Decrease...	0	1.0	2.1	3.0	Decrease...	0.1	0.8	1.1	2.2

From Table II we see that there is a gradual decrease in the diameter from year to year. We can only interpret this smaller diameter to mean forward growth of the nose. At birth, in all races, the transverse diameter of the bridge is much greater than the antero-posterior diameter. As growth continues in the Caucasian groups these diameters tend to become equal. The greatest amount of growth is in a forward (antero-posterior) direction. The distance between the canthi of the eyes is probably fixed at a very early age. On account of the absence of definite landmarks, the amount of lateral growth has been disregarded.

The minimum amount of this forward growth may be shown in the following way. Assuming the maximum diameter to be constant, the distance from this diameter to the apex of the bridge will increase and thus the antero-posterior growth may be determined. Assuming the diameter of 13.5 mm. as constant, the following amounts of forward growth are found:

	Boys	Girls
5 years	5.9 mm.	5.6 mm.
6 "	6.0 "	5.7 "
7 "	6.1 "	5.8 "
8 "	6.1 "	5.9 "
9 "	6.2 "	6.0 "
10 "	6.3 "	6.3 "
11 "	6.3 "	6.4 "
12 "	6.4 "	6.6 "
13 "	6.5 "	6.8 "
14 "	6.6 "	7.1 "
15 "	6.7 "	7.3 "
16 "	6.8 "	7.6 "
Forward growth	.9 "	2.0 "

We also notice that in the given time there is a greater decrease in diameters or a greater forward growth among the girls than among the boys. Assuming the form of nose to be the same for both sexes at an early age and a narrow high nose to be a form belonging to older individuals, it may be inferred that the girls for a given age are older physiologically than boys of the same age. Examining the measurements for each year we find that:

5-year-old girls correspond best with the 6-year-old boys
6 " " " " " " " 9 " " "
7 " " " " " " " 12 " " "
8 " " " " " " " 12 " " "
9 " " " " " " " 12 " " "
10 " " " " " " " 12 " " "
11 " " " " " " " 10 " " "
12 " " " " " " " 14 " " "
13 " " " " " " " 12 " " "
14 " " " " " " " 14 " " "
15 " " " " " " " 14 " " "
16 " " " " " " " 14 " " "

In almost every case the girls correspond to much older boys, but the fourteen, fifteen, and sixteen-year-old girls all correspond to the fourteen-year-old boys. We take this to mean that the girls are practically physiologically mature at fourteen while the boys continue to grow after this age. It is not meant to infer that these numerical data are final, but it is of interest that they agree with what has been found in other cases of growth.

These data suggest that a racial study of the nose should be based on accurate studies of its development during the period of growth. A comparison with the nose of anthropoid apes can be conducted successfully only by the same method. In a thorough study of this problem the form of the entire nose should receive consideration and casts should be substituted for the inaccurate lead wire cross sections. The number of cases should be greatly increased and since the annual growth is so slight the age unit could be increased from 1 to 4 years. A full appreciation of racial differences in anatomical structures must be preceded by a knowledge of the process of growth.

American Museum of Natural History,
New York City

THE SIO SHALAKO AT THE FIRST MESA, JULY 9, 1916

By WALTER HOUGH

With Commentary by Dr. J. Walter Fewkes

THE writer, while engaged in work in Tusayan for the United States National Museum, was present during the Katcina ceremonies and made some notes on one of them, which, though fragmentary, are appended. It is evident that shortly many of the Hopi ceremonies will cease, due to the increasing rapidity with which the influence of the white man is progressing.

The Sichomovi Sio Shalako is a Zuñi ceremony adopted by the Indians of Sichomovi, a village on the East Mesa.[1] The history of the adoption of the ceremony appears to be as follows:

Many years ago a delegation of Hopi went to Zuñi to attend the Shalako with the purpose of introducing it into Sichomovi. They are said to have received help from the Zuñi, but no charge was made. The ceremony is held pursuant to previous announcement given two years before but it has been performed in 1914 and 1916. Extensive preparations were necessary for the 1916 ceremony and as there were only two long rooms in Sichomovi sufficiently large, an addition was made to Connor's house and a new room built on another house. This work and the preparation of the costumes entailed great expense. The performers divided into four Shalako parties prepared at a house in the plain, Ushteoki, or Five Houses as it is called, and progressed slowly with many halts and songs across the valley to the mesa, arriving about seven o'clock in the afternoon. A group of Walpi priests awaited to receive them at the shrine below the mesa, having brought there all sorts of things in readiness, as Koyemshi masks, bundles of soapweed, stalks of new corn, rattles and drums. A row of conducting priests made a meal trail on the mesa along their course. One priest, Y'ate, carried a round deep basket, and the others, bags of *homgnumne* or prayer meal. They laid the *puhu*, or a cord with feathers attached, along the meal trail and expectantly awaited the arrival of the procession which they would allow to pass. The Shalako procession was led by Harry, the Snake Chief. First appeared the Fire god, Shulawitzi, an imp of blackness, bearing on his back a rack-shaped bundle of rabbits freshly killed, so that there must have been a rabbit hunt in the valley before the ceremony. Next came eight Shepaquin (Shaiastashas) with feathered and painted helmets with a parrokeet feather bundle atop; bearing in one hand a bunch of yucca leaves and in the other a bunch of cedar boughs. On the left wrist hung a rattle of sheep's scapula. These and all other priests were costumed, so far as can be judged, exactly as in the Zuñi ceremony. Some slight crudities in painting and other matters were noted and it is probable that an observer familiar with the Zuñi procedure would have made note of other variations from the Zuñi ceremony. The awe-inspiring Shalako next appeared, each with an attendant (Hakto) and accompanied with a crowd of Koyemshi as chorus. The Shalako danced four times around the plaza squatting four times each round upon a cloud design in prayer meal. The attendant priests in ordinary costume sprinkled the dancers at intervals with prayer meal and shouted directions. At the conclusion of the dance in the plaza the Shalako each went to the room which had been prepared for his reception. The Koyemshi removed their masks and painting, but the other celebrants retained their costume. The night drew on and a feast was brought in by women and laid on the floor of the ceremonial chamber. It was a bounteous spread consisting in the case observed of ten piles of *piki*, sixty loaves of bread, fifteen washbowls of sheep or goat stew, four buckets or pots of coffee, and some dishes of fruit. Some eighty persons, consisting of Navajo, bystanders, etc., sat down to the feast and there were loud sounds of deglutition. This scene was being repeated in the other ceremonial chambers in Sichomovi.

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[1] The first and so far as I know the only description of the Sio Shalako ceremony at Moki is by Dr. J. Walter Fewkes in the *Fifteenth Annual Report of the Bureau of American Ethnology*, pp. 296–7.

The chorus, consisting of those who had been Koyemshi, had gathered and in their midst was a big drum. The dirt floor of the room had been covered with muslin and three huge Rochester hanging lamps furnished the light. Deer skins folded to represent the deer, and with basket tray for head, hung on the wall together with blankets, and an especially large Navajo blanket hung back of the Shalako. Some chairs for guests were arranged on the south or left side; the other side was reserved for the women and children, who also filled the back of the room. The remains of the feast were removed but not before some grasping Navajo had filled bags with *piki* for a rainy day. Much smoking ensued until the guests were becoming impatient. Finally at the tap of the drum and sound of gourd rattles the chorus began. The dancers consisting of two Shalako attendants (Hakto), and two Saiastasha (Shepequin) began. At intervals a pair of dancers disappeared through the door and Koyemshi took their places but the Hakto remained. After a while one of the Hakto crept under the Shalako dress and made the figure move to the dance and gnash its teeth horribly. When guests fell asleep from fatigue the Hakto would strike them with the yucca bundle. The *monkohu* carried by the Hakto was a green rod having four breath feathers attached to it with cotton cords. Their dance kept up all night and in the morning the Shalako danced around the plaza four times and went away to Ushteoki and the Sio Shalako ceremony was over. At least seventy persons took active part in the dance as follows: ten village priests, four Shalako, four Hakto, eight Shepequin, one Shulawitzi or god of fire, and perhaps forty-eight Koyemshi. Dr. J. Walter Fewkes has pointed out the desire of the Hopi to adopt new Katcinas efficacious in bringing blessings. This, in part, explains the adoption of the Zuñi Shalako by the East Mesa Hopi and presents an instance of the transference of ceremonials, as well as giving an opportunity for studying comparative ceremoniology since undoubtedly changes from the Zuñi ritual have been introduced by the fertile genius of the Hopi. The song cycle is a composite of songs learned at Zuñi, songs of the Hopi Shalako, and songs invented for the occasion. The night songs were not counted but I estimated that there were eighty. The Zuñi Shalako number six;

said to represent the four points of the compass and above and below. The Hopi have adopted four in accordance with their quaternary scheme, which seems to have the primacy among this tribe. It is said, also, that the ceremony is announced two years in advance but it was not learned in what manner. The rites concerned with seating the Shalako in the dance room if any were practised were not observed.

Dr. Fewkes kindly drew upon his great store of knowledge of Hopi ceremonial and furnished the following interesting commentary on my observations:

> I have read your account of the Sio Shalako at Sichomovi almost literally by the light of my camp fire on the Mesa Verde, and as my time during the day is occupied by strenuous work on the ruin at Mummy Lake, I have little opportunity to comment upon it in a way I might wish to help you. Moreover my notes are all in Washington and I must rely mainly on my memory regarding the ceremony which you have described. I first heard of the Sio Shalako in 1894 and was then informed that it had not been performed for twenty years previously. As I remember the performance and compare it with your manuscript notes, I should judge the ceremony has lost much and gained nothing except by-plays in the interval between the celebration in 1894 and that you witnessed in 1916. I was told that this ceremony was introduced from Zuñi by Pautiwa, a man from Oraibi, who was visiting Zuñi with ten other Hopi. It is interesting to note that no other pueblo of the Hopi, including the native village of Pautiwa, ever presented this ceremony. The reason it is given in Sichomovi is that this pueblo was settled by clans who originally lived in Zuñi, or more accurately speaking, tarried in Zuñi for some time in their migration from the Rio Grande country in early times. Sichomovi is called by the Zuñi, a Zuñi town, on that account; and if you will consult the ritual of this village, published in my papers, you will find that the ceremonial dances there are Zuñi in character and the personators bearing distinctly Zuñi names.
>
> The personators arrayed themselves in a house several miles from the east mesa and marched to the town, costumed, along the old Zuñi trail. This house in the plain is owned by the Badger clan and the four chiefs belong to the Lizard, Badger, Patki and Asa clans or Tewa, in their region. The houses on the Mesa in which the ceremonies were performed belong to the eldest sisters of these four clans.
>
> The ceremony witnessed in 1894 began on July 6, when the chiefs and the majority of the chiefs on the East Mesa assembled in the Badger house in the plain and made prayer sticks or sacrificial offerings of a peculiar shape, numbering over two hundred. The masks used in this ceremony are in the keeping of the owner of this house and here the participants dressed themselves for the ceremony,

which occurred on the following day. At sunset on July 7th a procession composed of the Shalakos and other masked personages marched from this house up the Mesa side to the Badger house in Sichomovi. I have elsewhere described certain episodes of this rite which may be found in my account in the *Fifteenth Annual Report of the Bureau of American Ethnology*. There were several masked personages besides the giants in this procession, the most striking of which was, I think, the personator called the Big Head, an ancestral sun god of the Badger clan. There were complicated rites throughout the nights of July 7 and 8, with performances, elsewhere described, in the plaza of Sichomovi. As I remember them they were much more elaborate than those you described. The ceremony culminated on July 9, or rather about midnight of July 8th.

I recall on July 9 a ceremonial race called the Wawash Katcina, in which two large spruce trees, set upright at different ends of the court, were used. The nature of this ceremonial racing, in which many Katcinas appear, you will find in an account of it elsewhere published. One of the interesting episodes was a droll dance in which one of the Katcinas counts one, two, three, etc., during the racing. There were also a few of the mysterious Yaya or fire shamans, who treated a supposed sick man with charcoal. The Yucca Katcinas afforded much amusement to the assembled crowd by endeavoring to climb one of the spruce-trees. The "Delight Makers," as they are sometimes called, or the Tewa clowns, called Paiakayamu, were out in force. They imitated the disgusting habits of the Zuñi clowns and did unbelievable things, for which they were terribly punished by the Yucca Katcinas. I might go on with many details, but perhaps I have given enough to show the general character of this sensational show. Ordinary decency has perhaps eliminated much which was then performed. From your description I judge that the ceremony has lost its real significance and taken on much of the social character of the Zuñi Shalako in December.

There are one or two things, however, which I will add: First the Zuñi characters predominate for the reason of the kinship of the clans of Sichomovi. The whole ceremony, I believe, was like the Zuñi equivalent from which it was derived, a great sun festival; the Shalako Giants representing the sun birds or sky gods of the four cardinal points. As the Hopi have another greatly different sun festival in December, when the Sun drama is elaborately observed, the Badger clan perform their sun festival in the summer solstice month, June–July. I will not burden you with other suggestions about the meaning of the ceremony, but I have many notes which some time I hope to commit to cold type.

My belief is that Sio Shalako at Sichomovi is one of those pageants or show dances of which there are a few in the Tanoan pueblos on the East Mesa when the sensational rather than the serious intention is paramount. I should not for a moment rank it with Soyaluna, the Hopi celebration of the Sun Return, in Walpi at the December solstice, or with the Snake Dance in August, which are so characteristic of the Hopi ritual. The Hopi visitors to Zuñi were impressed with the showiness of the Shalako and brought it to Sichomovi as a festival to add to their already over-burdened system of show dances to amuse the inhabitants, possibly to attract visitors. Naturally they brought the race and to add to its attractiveness they perform it at the pueblo settled by Zuñi clans. It is in a way analogous to the Butterfly Katcina, as in both of them the serious kiva rites are absent or minimized.

U. S. National Museum,
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BOOK REVIEWS

METHODS AND PRINCIPLES

The Belief in God and Immortality. JAMES H. LEUBA. Sherman, French and Company: Boston, 1916. (Price $2.00 net).

Professor Leuba's book presents some most interesting investigations into the status of the belief in God and in immortality at the present time among men of science in the United States. Questionnaires were sent to 1,000 men of the physical and biological sciences listed in *American Men of Science*, to 200 historians from the membership list of the American Historical Association, to 48 teachers of sociology and 149 non-teaching sociologists chosen from the members of the American Sociological Society, and to 107 members of the American Psychological Association. Of the physical scientists 56 per cent. were non-believers in God, of the biological scientists 63 per cent. were non-believers, of the historians there were 52 per cent., of the teaching sociologists 75 per cent., of the non-teaching sociologists 45 per cent., and of the psychologists 76 per cent. By non-believers is meant agnostics, doubters, and disbelievers. The kind of God referred to is one to whom one may pray in expectation of receiving an answer, answer meaning more than the subjective, psychological effect of prayer. The percentages not believing in personal immortality were slightly smaller than the percentages not believing in God among all the groups of thinkers except possibly the psychologists. The higher percentages of non-believers among the psychologists, sociologists, and biologists is interpreted by Professor Leuba as being due to the fact that these groups are more accustomed to thinking of natural law applying to mental phenomena as well as to the phenomena of the physical sciences.

Another interesting feature of the investigation was the division of the groups into lesser and greater thinkers and a comparison of the non-believers in each division. In all groups the percentage of non-believers among the greater thinkers was considerably larger than among the lesser thinkers. Thus among the greater biologists 83 per cent. were non-believers in God while among the lesser biologists only 61 per cent. were non-believers. The differences between the lesser and greater thinkers of other groups were approximately the same as between the biologists, save in the case of the historians, where the believers among the lesser historians were about twice the number of believers among the greater historians. However, I think that the differences between the lesser and the greater thinkers should be interpreted only in general as showing more belief among the lesser thinkers than among the greater; and they should not be compared one with the other because the methods of separating the lesser from the greater cannot be shown to be the same in each case. Of the physical and biological scientists taken together 1 out of every 5.5 of the original lists were called greater, among the historians 1 out of every 3.7, and among the psychologists 1 out of every 3.8. The author does not give the figures from which I could tell what the proportions were for the physical scientists, the biologists or the sociologists. But even if the ratios were the same for each group, the original lists from which the greater were chosen cannot be shown to be sufficiently comparable.

An appreciation of Professor Leuba's very important and interesting investigations involves two considerations, one the framing of the questionnaire, and the other the statistical treatment of the results. The difficulties of framing a questionnaire on this subject are very great. The chief difficulties concerned the words "belief," "God," and "immortality." The reviewer thinks that the author has succeeded admirably in making questions on so complex a subject clear and simple; although it is a fact that the philosophers to whom the questionnaire was sent were unable to understand what the questions meant, or claimed that the terms were not adequately defined.

Concerning the statistical treatment criticisms should be directed at the method of sampling and the size of the samples. Professor Leuba picked his cases at random and gave excellent consideration to the very few who did not reply. There is, however, some fault to find with the size of some of his samples and with some of the conclusions he draws, although most of them and certainly his larger conclusions are thoroughly established. He chose 1,000 names from the 5,500 names in *American Men of Science* and to show that this sample is large enough he divides this list of 1,000 names into two divisions of 500 each and as the results tally for each of the divisions, the conclusion is that his sample is large enough. And he remarks (page 277):

> If, in the case of the scientists we may take the statistics of 1,000 as representative of 5,500 we may, a fortiori, accept the other statistics as representing the whole of each group, since in each the proportion upon which the investigation bears is larger than in the case of the scientists.

Since 1 in every 5.5 scientists is chosen, then if another group had, say, 1,100 names he would choose 200 (which is 1 in 5.5) as representative. By the same logic if another group had, say, 11 names he would choose 2 as representative. This is surely not the theory of errors. If there should be in the United States (or in the world) only 11 men in a particular science, this 11, although all there are, is yet a sample of a theoretically larger number.

This lack of use of the theory of errors leads the author into several minor conclusions not justified, I think. For instance, he stresses the fact that the psychologists are the only group where the percentage of believers in immortality (19.8 per cent.) is less than the percentage of believers in God (24.2 per cent.), explaining the conclusion by the peculiar nature of the science of psychology. But inasmuch as the sample included only 91 (or 93) replies, one questions if the difference is significant. Working out the error of the difference between the two percentages by the formula

$$\epsilon = \sqrt{\frac{p_1 q_1}{n_1} + \frac{p_2 q_2}{n_2}},$$

where the p's are the percentages, the error is 6 per cent. while the difference between the two percentages is only 4 per cent. In other words the chances of his conclusions being wrong are about 1 in 4. Also other differences between groups where the numbers are small should be interpreted cautiously by the reader.

It was impossible to test by certain statistical methods some of his conclusions, because unfortunately exact numbers are not given. It is decidedly to be regretted that full tables were not given. The author has endeavored, and successfully, to make his book popular and readable. But such tables could have been placed in the appendix. An illustration of this absence is noted particularly in his discussion of the very important question of the correlation between desire and belief in immortality, on which subjects questions were included in the questionnaire. The general result was that a good many more expressed a desire for immortality than expressed the belief and in some instances nearly all who had the belief had the desire. But only in a few cases does he give percentages and in only one case does he give numbers. In that one case I figured the correlation coefficient between desire and immortality to be 0.6, a fairly high correlation, working it out in the usual way, for the four-fold table. Methods of contingency and correlation could have been applied to advantage in several cases. I should repeat, however, that his more important conclusions are quite well established, and in nearly all instances his samples are large enough.

The author conducted two other investigations, among college students, which show that larger percentages of women are believers than men, that larger percentages of lower-classmen are believers than upper-classmen, and that from 40 to 45 per cent. of students leaving college are non-believers.

Not all of Professor Leuba's book deals with the statistics of belief. In Part I he discusses the origin and evolution of the idea of immortality and in Part III he discusses the present utility of the belief in personal immortality, which is held to be very small. These two parts certainly help greatly to give rich meaning to the very interesting statistics. They are also contributions to the study of immortality but not so clearly established by facts as the conclusions drawn from his statistics.

The belief in immortality (or continuation) appears among primitive peoples, the author holds, because of "the exteriorization of vivid memory images, the sense of presence, dreams and the visions of waking life," and is an "unavoidable interpretation, devoid of any moral significance, of facts directly perceived." This primitive belief does not evolve into the modern belief. There is a striking absence of continuity. The modern belief has a separate origin and is the creation of desire, a desire for the realization of moral ideas.

His treatment of the theory, it seems to me, establishes the existence of two important psychological factors determining the belief in life after death and the nature of that life, but neglects the importance of desire among primitive peoples, and magnifies the difference between the primitive idea and those of moderns. He states (page 42): "There is no evidence that the savage ever concerns himself with his own fate after death" and (page 123) that the primitive view of life after death is "devoid of any moral significance" and that "there is no likeness (between the primitive and modern views) whatsoever beyond the mere idea of continuation (of life after death)." In the light of these statements it is well to consider, as one instance, the beliefs of the Central Eskimo. They have a belief in a heaven, where there is no snow and ice and many deer, which is the reward of goodness and a belief in a hell where certain individuals go and are specifically punished by the deity for misdeeds done on earth. That desire among primitive peoples was abundantly translated into other than existing conditions seems true from the interpretations of myths by the psycho-analytic school of psychologists.

A full understanding of the nature of the belief in life after death among any one people at any one time demands a fuller consideration of a number of other factors than the two the author describes so well, as for instance, the general level of knowledge, the conditions of general religious belief, the conceptions of justice, the social and economic conditions shaping the desires for improved conditions in this life, in the form of a national kingdom or social reform, and perhaps many others. If one thinks of a belief in immortality as shaped by specific factors, the lack of continuity (if indeed this be proven) between primitive views and modern views breaking a certain evolutionary progress, does not assume such theoretical importance.

WILLIAM F. OGBURN

Arboreal Man. F. WOOD JONES, M.B., D.Sc., Professor of Anatomy in the University of London. Longmans, Green and Co.: New York, 1916. Pp. x, 230, 81 figs. (Price $2.40 net.)

Recent literature on the evolution of man has dealt chiefly with the changes brought about by the assumption of the upright gait. In *Arboreal Man* the effects of the erect posture have been a secondary consideration as compared to the influence of early life among the trees.

Evolution is assumed as a fact and all attempts at explaining the methods by which changes of structure came into being, were perfected, and handed down, are wisely disclaimed at the outset. The author defines his position by saying that change has been effected somehow, and somehow such changes have been inherited. Nevertheless if one does not partially accept the principle of "use and disuse" the arguments lose much of their force. Dr. Jones says (p. 6):

> Man comes from an arboreal stock. Two questions arise. When in the phylogeny of Mammals did this stock become arboreal and when did it give rise to a creature which we could possibly term human? The first question is capable of an approximate solution, the second is unanswerable, but we may say with regard to it that, if the term "humanoid" may be permitted, such a stock may have had a very early representation among the mammalian fauna.

As used and undefined by the author the term "humanoid" can lead only to confusion. If we regard it as a synonym of "primate" the answer will probably pass unchallenged. If, on the other hand, we regard it as a synonym of "anthropoid" or, as seems more justifiable, we take it to mean the more immediate ancestors of man, many paleontologists and anatomists will take exception. While the early separation of man from the remainder of the anthropoid stock is always a possibility, paleontological evidence at present does not justify an earlier separation than Miocene.

Mammals which become terrestrial sacrifice the amphibian and reptilian mobility of the limbs for stability in order to gain support for the body and facility of locomotion. This mobility once lost is never fully regained. Much is made of these facts in answering the question, when in the phylogeny of mammals did the "humanoid" stock become arboreal? In the anthropoid apes and more especially in man, we have the retention of this primitive mobility in the greatest degree. Only a very early arboreal life could prevent this member from becoming sacrificed to stability. Accordingly the ancestry of man is taken back to the Cynodont Therapsids (Reptiles) of the Triassic and Permian of South Africa. Here a scene comparable to that enacted by the climbing tailed Amphibians or Tree Frogs is depicted.

That the forelimb of Man and the Anthropoids represents a primitive condition in respect to mobility, number of digits, power of supination and rotation most anatomists will grant. But the view that for this reason, or any other reasons so far brought forward, the ancestry of man, in exclusion of the rest of the mammalia, must be pushed back into the very "dawn" of mammalian evolution will meet with some opposition. The alternative is that the generalized ancestor of all Metatherian and Eutherian mammals was very probably arboreal or sub-arboreal and had just these same characters of mobility of the arm and fore-arm as seen in Man and the Anthropoids. There is much evidence in support of this view. Adopting this theory we avoid the necessity for early separation of the Anthropoids from the remainder of the Mammals and Man from the Anthropoids. Nor do some of us see the necessity of deriving the generalized Mammal from an amphibious form as described above.

While demonstrating the anatomical primitiveness of the human forelimb Dr. Jones takes occasion to decry the ever present tendency to insinuate that in man we find the culminating form of evolution and perfected type. In reference to the brain this is true but in regard to other anatomical structures, such as limbs, the horse may justly claim this distinction.

One of the best chapters in the book is that on the "Human Foot" (p. 73). Some of the praise which has been lavished on the hand is transferred to the foot.

> If man should wish to point with pride to any organ the structure of which definitely severs him from all the other existing Primates, it is to the foot that he should point. If missing links are to be tracked successfully, the foot, far more than the skull or teeth will mark him as the Monkey or the Man.

It is most certainly true that this phase of Anthropology has been subordinated, perhaps necessarily. Man inherited an arboreal foot in which the middle digit was probably longest. Arboreal life had developed the hallux as a grasping member. Since becoming terrestrial man has paralleled the Horse in progressing towards a stage with a greatly reduced pes. That the small toe is a fast degenerating member no one can doubt. It is also true that the central axis of the foot as determined by the grouping of the interossei muscles has changed from the middle to the second digit. That such a change is possible is shown by the partly terrestrial Gorilla in which the central axis is variable, being sometimes the second and sometimes the third digit.

Hand feeding and the preparation of food have successfully conditioned the retraction of the snout region and a reduction in dentition. The newly acquired chin is regarded as a retrogressive character in modern man. If this can be conclusively proven it will be the final blow to the much doubted association of a certain type of chin with speech or a highly differentiated genio-glossus muscle.

One or more chapters are devoted to each organ or system of organs. Arboreal life has left its stamp on the vertebrae, pelvis, viscera and especially the respiratory system. The difficulties of locomotion in such a habitat has reduced the number of offspring among Primates. For those who are inclined to interpret Embryology in the terms of Phylogeny there is an interesting chapter on the "Human Baby" (p. 201).

One of the closing chapters deals with the failures of arboreal life. Among these are the Lemurs and Sloths, which became too highly adapted to arboreal life to change. Man's salvation and success are due to the fact that he was early arboreal but did not become a highly specialized arboreal form.

It is not to be expected in the present state of our knowledge that anyone would wholly agree with Dr. Jones and in some few cases it is to be doubted whether he would get a majority to agree with him. Taken as a whole, it is one of the best zoological treatments of man that has appeared. One thing especially commends it. It deals with evolution as a present-day process and not wholly confined to the past ages. The author's ability as a teacher is shown by the remarkably clear and interesting presentation of some complicated anatomical problems. It is in every sense a readable book and none of the evidences advanced can be passed over lightly.

Louis R. Sullivan

Rest Days: A Study in Early Law and Morality. HUTTON WEBSTER. The Macmillan Co.: New York, 1916. (Price $3.00.)

The accounts in this book of market days, of lunar days and calendars, of holy days, of evil days and of a miscellany of days under taboo the general reader who is curious about his own holiday observances will find enlightening. The student will find the compilations of facts and of critical conclusions and the full bibliographical notes of convenience.

Were the student to expect anything more, he would be disappointed. No systematic attempt is made to ascertain relations between the data given and other cultural facts, for example, between economic or religious facts. Ethnological and psychological assertions are made, to be sure, but between the assertion and its context there is little or no connection. It may be true, for example, that "the connection of the rest day with the farmers' pursuits is secondary rather than direct" (p. 102), but the pages which follow on market days supply no proof of that generalization. The author is inclined to assume that the belief in death pollution is earlier than the belief in death spirits (p. 63), but he does not say why. Nor does he say why he assumes that memorial rites for the dead or exorcising rites are "massed and assigned to particular times" to meet "a demand for order and precision" (p. 75). For one of his assertions or assumptions he explicitly promises evidence.

> It is reasonable to suppose that, with the deepening sense of social solidarity, observances once confined to the individual alone, or to his immediate connections, would often pass over into rites performed by the community at large. Some evidence tending to substantiate this opinion will be presented incidentally as the investigation proceeds (p. 7).

The promised evidence proves to be further assumption (pp. 25, 38–9, 63).

Other generalizations occur which are not even illustrated by facts. For example,

> The institution of taboo, in its individualistic aspects, has helped to nurse in man a sense of reverence, a power of self-restraint greatly needed under primitive conditions (p. 60).

We might almost infer from this passage, not to speak of others, that Dr. Webster had paid no attention to herd psychology and that it had never occurred to him that a sense of reverence might be in itself a primitive condition.

Consideration of the tabooed day as an expression of gregariousness Dr. Webster might well have found profitable. We should like to know, for example, what connection there is, if any, between communal rest days and communal activities. But the only way to ascertain the existence of such a connection or the lack of it would be not by psychological assertion, but by the statistical case method or something like it, by the Tylor-Steinmetz method of noting the concomitance in every culture of the facts in question or the lack of concomitance. The method of illustrating a generalization favored by Dr. Webster and others leaves us just where we were.

That Dr. Webster is a devout believer in cultural evolution he leaves us at no time in any doubt. His naïve use of the term "superstition" is the flag he waves incessantly in our face. But perhaps in no passage is his conviction as fully expressed as in the following:

> The ancient dwellers in the Arabian wilderness, who celebrated new moon and full moon as seasons of abstinence and rest, little dreamed that in their senseless custom lay the roots of a social institution, which, on the whole, has contributed to human welfare in past ages and promises an even greater measure of benefit to humanity in all future times (p. 246).

Unless, may we comment, the whole psychosis of crisis, of the meaning of which Dr. Webster seems unaware, should as a cultural manifestation go entirely into the discard. (Will magic, another psychological factor in rest days, modern culture has already fairly well discarded.)

But it is development, not discard, which appeals to Dr. Webster. He is one to whom, as he puts it,

> nothing is more interesting than the contemplation of that unconscious though beneficent process which has converted institutions, based partly or wholly on a belief in the imaginary and the supernatural, into institutions resting on the rock of reason and promoting human welfare (p. 307).

Who will say that the teleologist is a less happy man than the theologist—or a less fatuous?

ELSIE CLEWS PARSONS

El Hombre Fósil, HUGO OBERMAIER. Junta Para Ampliacion de Estudios e Investigaciones Cientificas. Madrid, 1916. Pp. XIV, 397. Numerous illustrations.

Dr. Obermaier is already well known to American anthropologists and prehistoric archaeologists as the author of *Der Mensch der Vorzeit*,[1] of which the present work might be considered as a revised and somewhat abridged Spanish edition. *El Hombre Fósil* is published as Memoir

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[1] For review of this work, see *Current Anthropological Literature*, vol. II, pp. 131–138, 1913.

Number IX of the *Comision de Investigationes Paleontologicas y Prehistoricas;* and in adapting his manuscript to the Spanish, the author has been fortunate in the collaboration of Ismael de Pan and Pablo Wernert. In the arrangement of subject matter, in bibliography and in the scope of the index, the present volume is superior to Professor Obermaier's earlier work. In the opinion of the reviewer, however, the index should have been presented as a unit instead of being divided into three parts.

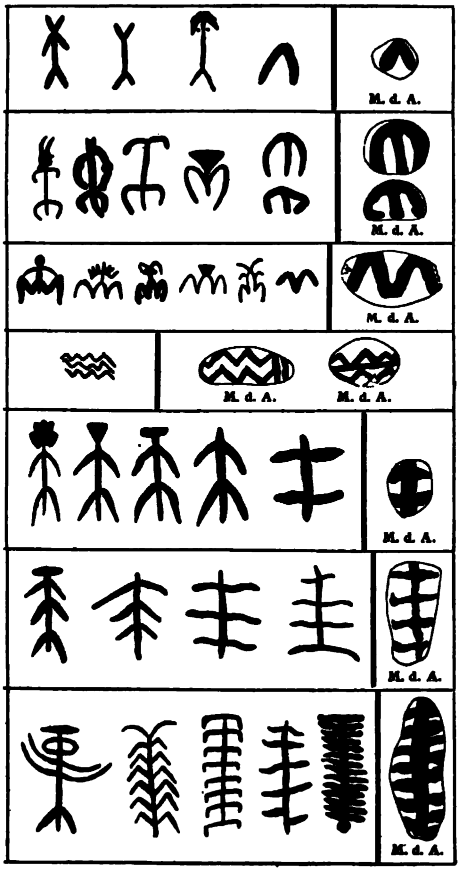

FIG. 60.—Comparison of Spanish petroglyphs with the designs on the painted pebbles of Mas d'Azil. Those to the left of the broad vertical bars are Spanish. *After Obermaier.*

One of the special merits of the volume is the fulness with which the latest discoveries have been discussed. The array of evidence in favor of a close correlation between the Spanish petroglyphs and the Azilian culture of France as represented in the painted pebbles of Mas d'Azil is a distinct contribution (fig. 60). The figures to the right of the broad vertical bars are from the painted pebbles, while those to the left are painted on the walls of Spanish caves and rock shelters. A number of colored plates add to the value and attractiveness of the volume.

George Grant MacCurdy

Socio-Anthropometry. B. L. Stevenson. Richard G. Badger, Boston: 1916. 153 pp.

The author of this little volume seems to labor under the impression that there is a considerable school, which she terms the Socio-Anthropometric, which teaches that if the laws of evolution hold in the physical or biological sense they also hold in the psycho-physical; hence, that a certain disposition, or characteristic type of mind, goes with a certain stature, head form, or color. She proposes to subject these teachings to a critical test to see if there is

> correspondence between likenesses and unlikenesses of sociologic and anthropometric types which can lead to the assumption that sociologic facts can be classified according to the postulates of physical anthropometry.
>
> Sociology postulates differences among men. Anthropology postulates differences among men. These differences are illustrated racially.

To test the case, she undertakes to classify the sociological and anthropological traits of "the three European races" and to apply the one to the other. The Teutonic, the Alpine, and Mediterranean races are the subjects of this discussion, but

> because general sociological and anthropological considerations in themselves are too extensive, national units are *ipso facto* prerequisite;

hence, England and Scandinavia are chosen to represent the Teutonic race, France and Russia the Alpine, and Italy the Mediterranean.

In the first group the author finds that two dominant strains have long worked to bring about the two-sided English character.

> The Saxon strain is expressed in love of home, of agriculture, of obedience to law and the democratic principle. The Norman strain shows itself in love of personal liberty, of adventure, of fine quality and truthful virtue, and the autocratic principle.
>
> The English mind is critically intellectual, the English disposition domineering and creative. The rationally conscientious attitude is the mould of the Briton.

The Scandinavians "belong to the category of morality." They express their strongest individuality in the spheres of religion and social rights, are rationalistic but dogmatic. The habit of thought is inductive. The type combines an austere character with a domineering but at the same time convivial disposition. In certain respects—such as their form of religion, in the nature of their government, their love of independence, self-reliance, and allegiance to the principles of logic—these two branches of the Teutonic race approach each other closely, but in other ways they are sociologically very far apart.

Referring to the findings of anthropometry, we are told that in some districts of Scandinavia broad heads, dark colors, and short stature prevail; that there has been mixture with Lapps in Norway, with Finns in Sweden, yet there is in Scandinavia, as a whole, "a racial dolichocephaly, tall stature, and fair coloring."

England's population is more consistently Teutonic in head index, and height, yet even here the traits of various peoples who have contributed to the population are still noticeable. In color, it is found that brunettes make up 31 per cent. of the population; stature varies from 156 cm. to 183 cm. (masculine), and head indices from 72 to 85; yet taken in large numbers they—the English—belong with the Scandinavians to the Teutonic race. To sum up:

> We can say that although physically they agree in type, sociologically they are very far apart in their expression of that type.

The two representatives of the Alpine race—Russia and France—are next considered. The Russian mind is found to be adaptive, imitative, and keenly perceptive, but lacking in will power. It is a logical mind of the deductive variety. Keenness and lucidity of thought predominate, at times to the exclusion of creative ability. The French are convivial in character and creative in disposition. Will power is manifest. The rationalism of the Teuton is absent. Intellectually they are deductive but critical in attitude.

> Sociologically the French and Russians are alike and unalike.

Turning to physical types, the author concludes that these two nations are rightly considered exponents of the Alpine, or Central European, type since in this respect—as well as in ethnologic and sociologic manifestations—"there are to be observed more similarities than dissimilarities." This despite the fact that Russia is "formed of Slavic, Asiatic, and Teutonic strains," that "in France more than in any of the countries instanced, except perhaps Russia, heterogeneity prevails," that the head index varies in France from 79 to 91.34, and stature from 154 cm. to 177 cm.

The Mediterranean race—represented by Italy—is, sociologically, a highly complex unit, due to the many elements which have contributed to its formation. The disposition is aggressive and instigative, the character a compound of two blends—austere and convivial—yet it is the dogmatic emotional which predominates in the larger character, a cool appraising quality of Italian judgment combined with hot impulsiveness of emotion. It is in the sphere of emotion, morals, and intellect that Italy has won her great sociological achievements.

Physically, Italy, like the countries already studied, shows great variation. In the north the Italian tends to be broad-headed, tall and fair; in the south he is long-headed, short and very dark.

> Thus it is seen that the anthropometric character of Italy's population presents a rather regularly culminating geographical order of color, height and head index, which *in toto* gives the general Mediterranean figure with its dolichocephalic head, its short stature and dark coloring.

From the foregoing the author is convinced that there are in Europe three distinct anthropologic types, but only two sociologic types,

> since the Alpine and Mediterranean types are not sharply differentiated from one another sociologically,

hence, she concludes that

> since neither racial nor national sociologic types hold consistently to anthropometric classification, the assumption is made that socio-anthropometry in postulating certain mental traits combined *sine qua non* with physical traits reduces to an absurdity.

The book appears to be an attack on a definite school of thought, but in order to establish this school, the author has, by implication at least, classed together all those who believe in physical, social or mental evolution; those who believe that environment and economic conditions affect physical man; those who believe in superior races and those who deny such superiority. Thus she classes Vacher de Lapogue, who holds the blond dolichocephal to be a member of a superior race, with Professor Boas, who consistently argues against the division of peoples into inferior and superior. It is true, Professor Boas recognizes that actual differences in races may force us to believe in some differences in their mental makeup; but he has also shown that there is no necessary correlation between race, language, and culture.

To the reviewer, it appears that the author has carefully constructed a "bogey figure," but out of such diverse materials that the finished article fails to resemble anything existing under heaven or on earth.

Even if we grant, for the moment, that her Socio-Anthropometric school does exist, we fail to see that the arguments here brought forward would invalidate its claims. What the author has done is to prove, by her carefully compiled physical data, that the five nations considered are made up of such heterogeneous types that it would be absolutely unsafe to try to correlate any known set of national mental traits with an average of the physical features. To take the physical average of a population as diverse as that of Italy, and to assign to it the Italian mental traits, as given by the author, is to place in the same category the tall brachycephals of Lombardy and Pedmont, and the short dolichocephals of Lucca and Sardinia. Such a procedure is no more justified than it would be to apply the same traits to the Perm group in eastern Russia, or to a considerable proportion of the population of Denmark.

Finally, the mental or sociological characteristics given for the various nations seem quite as open to question as is the average of physical data. Any argument which assigns the same mental traits to the Englishman of Yorkshire and the Irishman of Kerry needs no further refutation.

FAY-COOPER COLE

NORTH AMERICA

The Constitution of The Five Nations. ARTHUR C. PARKER. (New York State Museum Bulletin, No. 184, Albany, N. Y.: April 1, 1916).

Traditional History of The Confederacy of The Six Nations. DUNCAN CAMPBELL SCOTT, F.R.S.C. (Royal Society of Canada, Proceedings and Transactions, 3rd Series, vol. v., Ottawa, Canada, 1912, Section II, 195–246 pp.).

Civil, Religious and Mourning Councils and Ceremonies of Adoption of the New York Indians. REV. WILLIAM M. BEAUCHAMP. (New York State Museum Bulletin, No. 113, Albany, N. Y., June, 1907).

These three publications are considered here together because they deal with a common topic—the League of the Iroquois. They severally repeat old errors and so diffuse them broadcast under the patronage of learned institutions, and so the following strictures are made on the untrustworthy character of much of their contents, lest the unwary student be led into accepting misinformation for truth.

It must be noted that the second publication also forms a part of the contents of the first, in which the fact of its separate publication by Mr. Duncan Campbell Scott is not mentioned by the editor.

The third publication is interesting chiefly as compilation of texts, notes, comments and quotations rather than as a serious study and analysis of the complex institutions mentioned in the title of the work. Thus, for example, a long list of etymologies is quoted from Morgan, although a large majority of them are worthless; the texts and translations cited from Hale are not revised and corrected, although in very many instances they are misleading or erroneous. The want of accurate knowledge of the languages of the peoples to which the work refers made Dr. Beauchamp the victim of the palpable blunders in translation which his chief informant, the Rev. Albert Cusick, was prone to make.

Mr. Parker tells us that two main manuscripts form the basis of his publication. He fails, however, to point out the value of either manuscript, or to explain the significance of the serious conflict of statements of essential facts or events between the two; we should have been told the essential fact that the document prepared by the Committee of Chiefs of the Six Nations was prepared as a substitute for the Newhouse document, which the chiefs in council had thrice rejected as faulty in arrangement and erroneous or spurious in many of its statements. We are told that these two manuscripts were "discovered" in 1910 on the Six Nations reserve, Ontario, Canada; it is however a fact that the Newhouse "Constitution," although in much briefer form, had been known since 1880, for in that year a copy of it had been left by its compiler on the Cattaraugus reservation for safe-keeping; the document of the chiefs of the Six Nations was prepared in the early spring of 1900 while the present writer was a guest of the late chief John Arthur Gibson, one of the Committee of Chiefs.

Again, Mr. Parker should have explained also that this document of the Committee of Chiefs, supplemented, however, by a portion of the native matter appearing in Hale's *The Iroquois Book of Rites*, 1883, had been published as early as 1912, by Duncan C. Scott, F.R.S.C., in the *Proceedings and Transactions of the Royal Society of Canada* as noted above.

The following comments deal chiefly with the contents of Bulletin 184, by Mr. Parker. The statement that Rev. Albert Cusick was

> employed for more than a month in correcting the Newhouse manuscript until he believed the form in which it is now presented fairly correct and at least as accurate as a free translation could be made

is contrary to the facts. The Newhouse manuscript has appeared in a number of varying versions, which were one and all originally recorded in the English language. But in 1897–8 the present writer induced Mr. Newhouse to undertake with him the translation of the best of these, after recasting and rewriting certain portions of the various sections. This work was undertaken in order to preserve in Mohawk terms of juridical, governmental, and of ritualistic import, but not for publication in the material as found. So the matter of this document is in no accepted sense a "free translation" of a native text. It is indeed unfortunate that the translated matter quoted by both Dr. Beauchamp and Mr. Duncan from Hale's *Iroquois Book of Rites* should have been used without radical and essential corrections in the forms and translations of vital portions of the Ritual, for the renderings of entire sections are faulty and misleading, and often quite contrary to the intent of the originals.

These editors were apparently quite unaware of the serious blunders in translation and statement they were unconsciously diffusing as sources of further error. A specimen of the untrustworthy character of much of the material in question may be cited here. The provision for a private visit of condolence and sympathy to the death-lodge three days after the burial the body of a dead chief (Duncan, *op. cit.*, Parker, *op. cit.*, 109) is to the point; this visit is to be made by a delegation from the "cousin" sisterhood of tribes, for the purpose of comforting the bereaved family and kindred with the substance of *eleven* (not *thirteen*) of the Fourteen Themes (Ne" Adondakshah) of the Requickening Address; what follows is intended to be the caption of this hearthside address and is in the words following:

> The beginning of the Condolence Ceremony used immediately after the death of a chief (or Lord) and which is subsequently followed by the thirteen ceremony called 'At the Wood's Edge.'

It is clear that the thirteen sections of the address cited here are for the formal public function and so are not at all in the form suitable for use at the private lodge hearthside. Besides there is no such thing as a thirteen Ceremony called "At the Wood's Edge." Mr. Parker quotes eleven strings of wampum, although he cites thirteen sections just as does Mr. Duncan; but the interpolated remarks in section 3, to wit:

> The foregoing part of the Condoling Ceremony is to be performed outside of the place of meeting. Then the bereaved will appoint two of their chief warriors to conduct the four brothers into the place of meeting,

should have been a loud hint to the editors that they were quoting wrong matter. This is a confusion of a private visit with a public function. Careless proofreading permitted an undue number of inexcusable misprints to appear. Skanawatih's (9 and 13) or Skanawita's (30) is evidently printed for Dekanawida's. Abbreviations share in these errors of proofreading; *SPW* (pp. 52, 53, 54 and elsewhere) evidently should be *LPW*. Too much credence is placed in the authenticity of the so-called Passamaquoddy wampum traditions, which are of course concerned with the activities of the "Iroquois" settled at Caughnawaga and elsewhere on the St. Lawrence, Canada, and so have little or no bearing on the early history of the "Five Nations" of New York.

The number of federal chiefs was not fixed at fifty. This is an unhistorical number which is known only within the last century; it arose from a misinterpreted tradition concerning the episode in which one Bearfoot was a chief figure. The first traditionally authentic number is 47, to which in later times were added two Seneca chiefs, making 49 as the highest authentic roll of official titles of federal chiefs. Authentic tradition is silent as to the original number. It is usually found by adding together the several tribal lists.

It is not traditional nor historical to say that for "many generations" the knowledge of "each law or regulation" of the League had been "preserved" by means of "a collection of wampum belts and strings," as the traditions published along with this statement clearly show, for these inform us that the founders of the League knew apparently little, if anything symbolic, about wampum, but rather something definite about "elderberry twigs" and "quills." Hence, the further statement (p. 8) that "Several of the wampum belts in the New York State Museum are Constitutional belts or memorials" seems indefensible, if it is desired to suggest that they were used by the founders of the League. Dr. Beauchamp (*New York State Museum Bulletin*, No. 41, vol. 8) has reached conclusions in accord with this remark.

The statements on page 13 concerning the use of the word "Longhouse" are based on misinformation and superficial observation. In no Iroquois tongue does the native name of the League signify "Longhouse." There is, therefore, never any confusion between the native names for the "Long House," the usual place of assembly, and that for the institution, called the League or Confederation. Trustworthy and discriminating interpreters and informants would so translate these native terms as to emphasize this important difference. The native name of the League is *Gano*^{n}*‘syoñ'ni'* (with initial K in some dialects) and signifies "The Extended Lodge," *i. e.*, the Lodge that is Extensive; that is, spread out far, especially lengthwise. But the native name for the ordinary public assembly lodge is *Gano*^{n}*'‘ses* (in some dialects the last *e* becomes an *i*) and means "The Long Lodge," commonly shortened to "Long-house"; and so there is no reason for confusion here. So it is a gratuitous remark to say that Handsome Lake destroyed "the old religious system," for there is nothing to show its truth; the great religious festivals, all antedating the time of Handsome Lake, are today still in vogue on the Grand River reservation and elsewhere.

The references (p. 14 and elsewhere) to the "Crooked Tongues" are due to hazy ideas about the facts in the matter. There is no historical or traditional evidence, known to the present writer, showing that the Neuter Nation, so-called, ever had lands lying northeast of Lake Ontario, or that a Huron village called Kahanayen was situated on them. The land in question was probably Huron territory for Dekanawida's date. Evidently, the name "Crooked Tongues" is used as a substitute translation of the Huron and Neuter name Attiwendaronk (Hatiwendaronk) which signifies "Their speech is awry," *i. e.*, "They speak a language slightly different (*from the norm of ours*)." The source of the utter confusion of names and places probably arose from misunderstanding certain information which the present writer many years ago gave to Mr. Newhouse concerning the early inhabitants of what is now Ontario, Canada. This information contained the suggestion that Dekanawida was very probably a naturalized Huron captive among the Iroquois. Singularly, the Huron tribes do not figure in the traditions relating the events leading to the formation of the League. So the comments on page 15 concerning Dekanawida's troubles with his own people are probably fiction, and especially so is the alleged conversation carried on with Mohawk people; the Mohawk did not know the Wyandot (Huron) as the "Crooked Tongues," as the statement on page 14 implies. After crossing the Lake, Dekanawida was not in Mohawk hunting ground, but in that of the Oneida or Onondaga, being a long distance from the Mohawk villages.

There is also confusion between an alleged "immutability" in contradistinction from the asserted "continuity" of the institutions of the League. Amendments to already existing laws are monotonously frequent in the traditions. Again, it is not true that the term *oñgwe'ʻhoñweʻ* implies any notion of peculiar "originality" of descent or of "superiority" of race. This compound term signifies "native man," and is a limitation of the general term *oñ'gweʻ*, "man-being," *i. e.*, any living being having human attributes, the man of myth, to the "native" strictly ***human*** man. The Indian knew no race other than his own, for this term is also applicable to the Eskimo. Any other view is untenable.

There is no justification for the substitution of the words, "the soft, white, feathery down of the globe thistle," for the original false translation, "The Great Belt of White Wampum," of the native term, Jonodakenrahkowa, of the Newhouse manuscript of 1898, for these terms in no sense correct the incorrect rendering and they wholly miss the expressive symbolism of the native expression. The unhistorical character of the list of fifteen "original" clans, appearing in section 42 on page 42, in which seven are spurious, is clear to any careful student of the early clans of the Five Nations, for even one of the most important—the Wolf—has been suppressed without comment. The last three probably owe their existence to *otosis*—originating in the mishearing of names. The so-called "Ball" clan is a Hawk clan; the Opposite-Side-of-the-Hand is a Wolf clan; and the "Wild Potatoe" is a "Tuber Duck" clan; or is possibly due to the mishearing of a dialectic pronunciation of the name for Plover.

The Mohawk text first published by Hale in his *Iroquois Book of Rites*, then by Rev. Dr. Beauchamp, and finally by Dr. Scott (*op. cit.*, p. 238) gives no warrant for the astonishing statements (*Bulletin*, No. 184, pp. 27–28) concerning the clan towns, namely, "Now the party passed through these places" and "All these places are in the Mohawk country." The native text already mentioned states that some of these towns belong to the Wolf clan, some to the two Turtle clans, and some to the Bear clan. It would have been thus unnecessary for the 'party' to march through these towns, for they were evidently not all in "the Mohawk country" but dispersed among all the tribes there represented; and the clans-people present are severally addressed as coming from these several towns, and it is further stated, and this is important, that these *four* clans made up the number of clans "in ancient times." The last statement bears on what has been said about the clan list in section 42.

In article 63 the words "two sons," which occur several times, represent a wrong translation of a native term of relationship which signifies, in this place, "parent and offspring," usually translated, "Father and Son," or "Mother and Daughter." Section 19 is scarcely more than an expansion of section 18, and its provision for an independent "Council of War Chiefs" is unhistorical; too many councils of coördinate jurisdiction would result from it. Sections 55, 56 and 57 are confused and so in their present form unhistorical. The alleged provision for the dissolution of the League is indefensible; the provisions of Sections 56 and 57 are inconsistent one with another, and these in turn are traversed by the ordinances set forth in the second paragraph on page 103 (which is a part of the Committee's document).

The song set forth in Section 105 is certainly not what its caption, "The Installation Song," represents it to be; there is, indeed, no such song. This song was personal to the first Wathatotarho (Adodarho), and so it is not an installation song of to-day. The first line should have been translated, "I possess a fine thing," instead of "It is good, indeed."

On page 91 the statement is made that circumcision was practised on one of the founders of the League. This statement of course is inaccurate, as the Iroquois performed no such rite. The native words have quite a different and symbolic meaning.

The expressions, "white lion," "fire dragon of discord," and "white panther," on page 103, are attempts at translating native words which together are the name of one of the primal man-beings of Iroquoian myths of creation who was therein the personification of Discord. The literal meaning of his name is "the white-bodied meteor or flying-dragon." He brought about discord in heaven (the sky-world) which resulted in the complete metamorphosis of beings. So to translate his native name by the words "white lion" is erroneous, for it does not express the ideas intended in the text. The original manuscript in 1900 contained the words "white lion" at this point. So the present writer pointed out that such a rendering was inaccurate because the early Iroquois did not know the lion but did know the meteor. And he suggested further that as the "Master or God of Discord" was here intended, the better expression in English would be "the white-bodied meteor, the white-bodied fire-dragon or panther, of discord"; the suggested correction was approved by Chief Abram Charles and the late Chief John Arthur Gibson of the Canadian Six Nations, to whom it was made. The native term in note 3 on page 103 signifies "Death, or the Destroyer," a very different idea from the one sought to be expressed in the text.

In regard to the ownership of land, the latter part of Section 42 contradicts Section 44. The two main documents (compare pp. 11 and 12 with 98 and 47, respectively) disagree flatly as to usage in color symbolism. And both are in error in regard to the meaning of the native term "*Ska-no-dah-ken-rah-ko-wah,*" which signifies "the very great white mat (foundation)," for both erroneously translate "belt" where they should render "mat." They differ entirely as to its color; the Committee's statement being the correct one. The statement of the Committee's document (p. 103, 2d paragraph) in regard to the community of hunting-grounds is correct; but the Newhouse assertion (p. 45, Sec. 57) is of course inaccurate, because it limits the right to the "one bowl" containing "a beaver's tail" (*i. e.*, the common game preserve) to the "Lords of the Confederacy." His use of the term "cooked" beaver's tail, shows clearly that he has still to learn the meaning of this wise and benevolent provision of the founders of the League.

Sections 99 to 104 do not belong in a work of this kind. And Sections 105 and 106 are wrongly labeled and are not a part of the Constitution of the League. Section 107 does not belong here; many things naturally were taken for granted as well-known common law. Sections 108 to 117, private "Funeral Addresses," do not form any part of the Constitution of the League, and so are out of place here; their crudity and naïveté should have excluded them from consideration.

Section 64 is certainly not a law or ordinance; Section 72 is merely an expansion of 71; Section 73 is contradicted by 57; Sections 19, 59 and 98 deal with the same matters and so in their present form are unhistorical, and so is the last part of Section 79. Sections 74 and 77 are unauthentic; 87 is largely a duplicate of 83; and Sections 82 and 89 are not parts of the "Constitution," so-called. And Sections 85 and 88 are entirely contrary to the basic principles of the League as founded by Dekanawida. Sections 93 to 96, having been translated from Lafitau's *Moeurs*, *etc.*, by the present writer for Mr. Newhouse, are not traditionally parts of the "Constitution."

Of the footnotes on the pages from 65 to 107, thirty-three are erroneous and misleading.

It is noteworthy that the Secretaries of the Committee of Chiefs of the Six Nations Council admit that the traditions which they recorded have been "much modified" by several causes. But these annalists failed to detect in some notable instances the elements which have been assimilated by the League traditions from their mythic and other tales. Such, for example, are the following: the notion of "the white stone canoe" or the "marble canoe," and the "Ohsinoh" incidents. Now, the "stone" or "flint" canoe belongs to the cycle of stories which relate to the Winter God whose means of travel on water is a block of ice, which is poetically transformed into a "canoe." So this episode does not belong to the Dekanawida legend. Mr. J. V. H. Clarke (*Onondaga*, I, 1849) records the Dekanawida story, but he writes "*white* canoe" only; the original Dekanawida canoe was probably a birchbark canoe. But tradition has expanded "white" into "white stone" as suggested above. Moreover, Clarke mentions one *Ho-see-noke* as "A kind hearted, merry chief" who in behalf of the Council comforted the vexed mind of Hiawatha; but it is found that the Newhouse story makes this man whose name is in slightly different spelling, thus Ohsinoh (Osinoh) page 18, "a famous shaman" who destroys Hiawatha's daughters by "evil magic arts." Here we find a complete transformation of a man and his deeds.

It is to be noted that an extra verse has been unwarrantably added to the well-known "Six Songs" (Duncan, 239 p.), which derive their common name from the fact that just "six" verses constitute this chant; it is also quite erroneous to say, following Hale, "the Hymn called 'Hail,'" for this designation rests on a mistranslation of the native name and on a worse misapprehension of the real import of the chant.

Other misprints are Jiknosahseh (p. 91), Djikonsase (p. 90), and Djikonsă'se' (p. 71), for Djigo^{n}'să''sĕ^{n}'. It may be said here that there is no evidence that this person, the so-called Peace Woman, was in any sense a character "in Iroquois mythology." An examination of the provisions of Section 91 shows that they are in serious conflict with those of Section 59.

The inept and wholly whimsical comments on the pictographs published on page 111 indicate that here the editor was the dupe of cocksure but ignorant informants. The utterly fanciful character of these comments is indeed emphasized by the remarkable fact that this page of pictographs and comments appears as an inset in a misnamed and badly garbled summary of the "Re-Quickening Address of the League Ritual of Condolence and Installation." These pictographs from number 4 refer serially to the paragraphs of the summary beginning with number 1 on page 110, although the editor seems unaware of this interesting fact. It is to be noted that this *Address* deals with the *tribes* of the League but not with the *clans* of the League; the printed comments are incorrect in this respect.

The two pictographic groups of parallel lines respectively refer to the 'Father' and 'Mother' side of the League structure—the four representing the 'Mother' side and the three, the 'Father' side; the four lines represent the Oneida, the Cayuga, the Tuscarora, and the Delaware, the so-called 'Four Brothers'; and the three lines, to the Mohawk, the Seneca, and the Onondaga, the so-called 'Three-Brothers.' The prostrate figure indicates that the 'Three Brothers' are the mourning side, and the first erect figure shows that the 'Four Brothers' are the celebrant side according to the Ritual and so are not mourning in the ritualistic sense,—a needful distinction.

Then the pictographs marked 4, 5, and 6 represent the three Acts or Words spoken "At the Wood's Edge," the key words being respectively "wipe away the tears," "clear out the ears," and "remove the choking from the throat"; these acts are mentioned in paragraphs marked 1, 2, and 2, on page 110. Figure 7 (4 on p. 110) does not hold a "sun" as here stated, but "a cup," containing "the waters of pity"; figure 8 (5 on p. 110) does not denote "a bench with four legs," but rather the "seat" or "the reed mat," said to be stained with blood; figure 9 (6 on p. 112) denotes "the darkness" of grief; figure 10 (7 on p. 112) denotes "the loss of the sky" from grief; figure 11 (8 on p. 112) denotes "the loss of the sun" from grief; figure 12 (9 on p. 112) denotes "the grave," *i. e.*, "the upturned earth"; figure 13 (idea not on p. 112) denotes "20 strings" as the price exacted for a homicide, which is the circle of protection for the two groups of parallel lines; figure 14 (11 on p. 112) denotes "the reverence due the person of the woman"; figure 15 (wanting on p. 112) denotes "the malific powers of the earth"; figure 16 (wanting on p. 113) denotes "the obligation of mutual respect and service"; figure 17 (wanting on p. 113) denotes "the torch of announcement, or of notification"; and figure 18 (wanting on p. 113) denotes "the doorway," *i. e.*, the "end of the address." These brief strictures show how much real harm is done by the rush to publish unstudied material, no matter by whom.

But to enumerate the redundancies, the contradictions, and the misconceptions in Mr. Parker's *Bulletin* would require a volume larger than the publication in question. It is most unfortunate for the cause of historical truth that great institutions insist on publication at the expense of study and accuracy. It may be mentioned that this publication of Mr. Parker has been most unfavorably reviewed by Dr. Goldenweiser in volume 18, no. 3, pp. 431–436, of *The American Anthropologist.* I have purposely not given out this unfavorable estimate of Mr. Parker's recent work until it had been reviewed by one whose motive Mr. Parker might not question.

J. N. B. HEWITT

SOME NEW PUBLICATIONS

Barrett, S. A. The Washo Indians. (Bulletin of the Public Museum of the City of Milwaukee, vol. 2, no. 1, May 10, 1917); pp. 1–52, pls. 1–13.

Bates, Oric, *editor*, and Sterns, F. H., *assistant editor.* Harvard African Studies, 1: Varia Africana, 1. The African Dept. of the Peabody Museum of Harvard University, Cambridge, Mass., 1917. 271 pp., 2 + 18 + 1 + 7 + 3 + 1 + 26 + 1 pls., numerous figs.

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DISCUSSION AND CORRESPONDENCE

DO WE NEED A "SUPERORGANIC"?

NOTHING irritates a student of culture more than to have the methods of the exact sciences flaunted in his face as a salutary antidote to his own supposedly slipshod methods. He feels that he deals with an entirely different order of phenomena, that direct comparison between the two groups of disciplines is to be ruled out of court. It is some such irritation that seems to have served as the emotional impetus of Dr. Kroeber's very interesting discussion of "The Superorganic" (pp. 163–213 of this volume). Many anthropologists will be disposed to sympathize with him and to rejoice that he has squarely taken up the cudgel for a rigidly historical and anti-biological interpretation of culture. His analysis of the essential difference between organic heredity and social tradition is surely sound in the main, though doubts suggest themselves on special points in this part of the discussion. The common fallacy of confounding the cultural advancement of a group with the potential or inherent intellectual power of its individual members is also clearly exposed. There is little in Dr. Kroeber's general standpoint and specific statements that I should be disposed to quarrel with. Yet I feel that on at least two points of considerable theoretical importance he has allowed himself to go further than he is warranted in going. I suspect that he may to some extent have been the victim of a too rigidly classificatory or abstractionist tendency.

In the first place, I believe that Dr. Kroeber greatly overshoots the mark in his complete elimination of the peculiar influence of individuals on the course of history, even if by that term is understood culture history, the history of social activities with practically no reference to biographical data as such. All individuals tend to impress themselves on their social environment and, though generally to an infinitesimal degree, to make their individuality count in the direction taken by the never-ceasing flux that the form and content of social activity are inevitably subject to. It is true that the content of an individual's mind is so overwhelmingly moulded by the social traditions to which he is heir that the purely individual contribution of even markedly original minds is apt to seem swamped in the whole of culture. Furthermore the dead level of compromise necessitated by the clashing of thousands of wills, few of them of compelling potency, tends to sink the social importance of any one of them into insignificance. All this is true in the main. And yet it is always the individual that really thinks and acts and dreams and revolts. Those of his thoughts, acts, dreams, and rebellions that somehow contribute in sensible degree to the modification or retention of the mass of typical reactions called culture we term social data; the rest, though they do not, psychologically considered, in the least differ from these, we term individual and pass by as of no historical or social moment. It is highly important to note that the differentiation of these two types of reaction is essentially arbitrary, resting, as it does, entirely on a principle of selection. The selection depends on the adoption of a scale of values. Needless to say, the threshold of the social (or historical) *versus* the individual shifts according to the philosophy of the evaluator or interpreter. I find it utterly inconceivable to draw a sharp and eternally valid dividing line between them. Clearly, then, "individual" reactions constantly spill over into and lend color to "social" reactions.

Under these circumstances how is it possible for the social to escape the impress of at least certain individualities? It seems to me that it requires a social determinism amounting to a religion to deny to individuals all directive power, all culture-moulding influence. Is it conceivable, for instance, that the dramatic events that we summarize under the heading of the Napoleonic Period and which are inextricably bound up with the personality of Napoleon are a matter of indifference from the point of view of the political, economic, and social development of Europe during that period and since? Would the administration of the law in New Orleans be what it now is if there had not existed a certain individual of obscure origin who hailed from Corsica? It goes without saying that in this, as in similar cases, the determining influence of specific personalities is, as a rule, grossly exaggerated by the average historian; but a tendency to deprecate too great an insistence on the individual as such is not the same thing as the attempt to eliminate him as a cultural factor altogether. Shrewdly enough, Dr. Kroeber chooses his examples from the realm of inventions and scientific theories. Here it is relatively easy to justify a sweeping social determinism in view of a certain general inevitability in the course of the acquirement of knowledge. This inevitability, however, does not altogether reside, as Dr. Kroeber seems to imply, in a social "force" but, to a very large extent, in the fixity, conceptually speaking, of the objective world. This fixity forms the sharpest of predetermined grooves for the unfolding of man's knowledge. Had he occupied himself more with the religious, philosophic, aesthetic, and crudely volitional activities and tendencies of man, I believe that Dr. Kroeber's case for the non-cultural significance of the individual would have been a far more difficult one to make. No matter how much we minimize exaggerated claims, I fail to see how we can deny a determining and, in some cases, even extraordinarily determining cultural influence to a large number of outstanding personalities. With all due reverence for social science, I would not even hesitate to say that many a momentous cultural development or tendency, particularly in the religious and aesthetic spheres, is at last analysis a partial function or remote consequence of the temperamental peculiarities of a significant personality. As the social units grow larger and larger, the probabilities of the occurrence of striking and influential personalities grow vastly. Hence it is that the determining influence of individuals is more easily demonstrated in the higher than in the lower levels of culture. One has only to think seriously of what such personalities as Aristotle, Jesus, Mahomet, Shakespeare, Goethe, Beethoven mean in the history of culture to hesitate to commit oneself to a completely non-individualistic interpretation of history. I do not believe for a moment that such personalities are merely the cat's-paws of general cultural drifts. No doubt much, perhaps even the greater part, of what history associates with their names is merely an individually colored version of what they found ready to hand in their social, philosophic, religious, or aesthetic milieu, ***but not entirely***. If such an interpretation of the significance of the individual introduces a repugnant element of "accident" into the history of culture, so much the worse for the social scientists who fear "accident."

The second point in Dr. Kroeber's essay that I find myself compelled to take exception to concerns his interpretation of the nature of social phenomena. If I understand him rightly, he predicates a certain social "force" whose gradual unfolding is manifested in the sequence of socially significant phenomena we call history. The social is builded out of the organic, but is not entirely resolvable into it, hence it implies the presence of an unknown principle which transcends the organic, just as the organic, while similarly builded out of the inorganic, is not resolvable into it but harbors a new and distinctive force that works itself out in organic phenomena. I consider the analogy a false one. Moreover, I do not believe that Dr. Kroeber has rightly seized upon the true nature of the opposition between history and non-historical science.

The analogy is a false one because, while the organic can be demonstrated to consist objectively of the inorganic plus an increment of obscure origin and nature, the social is merely a certain philosophically arbitrary but humanly immensely significant *selection* out of the total mass of phenomena ideally resolvable into inorganic, organic, and psychic processes. The social is but a name for those reactions or types of reaction that depend for their perpetuation on a cumulative technique of transference, that known as social inheritance. This technique, however, does not depend for its operation on any specifically new "force," but, as far as we can tell at present, merely implies a heightening of psychic factors. No doubt the growth of self-consciousness is largely involved in the gradual building up of this technique of social transference. While we may not be able to define satisfactorily the precise nature of self-consciousness or trace its genesis, it is certainly no more mysterious a development in the history of mind than earlier stages in this most obscure of all evolutions. In short, its appearance involves no new force, merely a refinement and complication of an earlier force or of earlier forces. Hence social activities, which I define as a selected group of reactions dependent at last analysis on the growth of self-consciousness, do not result from the coming into being of a new objective principle of being. The differential characteristic of social science lies thus entirely in a modulus of values, not in an accession of irresolvably distinct subject matter. There seems to be a chasm between the organic and the inorganic which only the rigid mechanists pretend to be able to bridge. There seems to be an unbridgeable chasm, in immediacy of experience, between the organic and the psychic, despite the undeniable correlations between the two. Dr. Kroeber denies this *en passant*, but neither his nor my philosophy of the nature of mind is properly germane to the subject under discussion. Between the psychic and the social there is no chasm in the above sense at all. The break lies entirely in the principle of selection that respectively animates the two groups of sciences. Social science is not psychology, not because it studies the resultants of a superpsychic or superorganic force, but because its terms are differently demarcated.

At this point I begin to fear misunderstanding. It might almost appear that I considered, with certain psychological students of culture, the fundamental problem of social science to consist of the resolution of the social into the psychic, of the unraveling of the tangled web of psychology that may be thought to underlie social phenomena. This conception of social science I have as much abhorrence of Dr. Kroeber as.

There may be room for a "social psychology," but it is neither an historical nor a social science. It is merely a kind of psychology, of somewhat uncertain credentials, for the present; at any rate, it is, like individual psychology, a conceptual science. It is quite true that the phenomena of social science, as claimed by Dr. Kroeber, are irresolvable into the terms of psychology or organic science, but this irresolvability is not, as Dr. Kroeber seems to imply, a conceptual one. *It is an experiential one.* This type of irresolvability is *toto caelo* distinct from that which separates the psychic and the organic or the organic and the inorganic, where we are confronted by true conceptual incommensurables.

What I mean by "experiential irresolvability" is something that meets us at every turn. I shall attempt to illustrate it by an example from a totally different science. Few sciences are so clearly defined as regards scope as geology. It would ordinarily be classed as a natural science. Aside from palaeontology, which we may eliminate, it does entirely without the concepts of the social, psychic, or organic. It is, then, a well-defined science of purely inorganic subject matter. As such it is conceptually resolvable, if we carry our reductions far enough, into the more fundamental sciences of physics and chemistry. But no amount of conceptual synthesis of the phenomena we call chemical or physical would, in the absence of previous experience, enable us to construct a science of geology. This science depends for its *raison d'être* on a series of unique experiences, directly sensed or inferred, clustering about an entity, the earth, which from the conceptual standpoint of physics is as absurdly accidental or irrelevant as a tribe of Indians or John Smith's breakfast. The basis of the science is, then, firmly grounded in the uniqueness of particular events. To be precise, geology looks in two directions. In so far as it occupies itself with abstract masses and forces, it is a conceptual science, for which specific instances as such are irrelevant. In so far as it deals with particular features of the earth's surface, say a particular mountain chain, and aims to reconstruct the probable history of such features, it is not a conceptual science at all. In methodology, strange as this may seem at first blush, it is actually nearer, in this aspect, to the historical sciences. It is, in fact, a species of history, only the history moves entirely in the inorganic sphere. In practice, of course, geology is a mixed type of science, now primarily conceptual, now primarily descriptive of a selected chunk of reality. Between the data of the latter aspect and the concepts of the former lies that yawning abyss that must forever, in the very nature of things, divorce the real world of directly experienced phenomena from the ideal world of conceptual science.

Returning to social science, it is clear that the leap from psychology to social science is just of this nature. Any social datum is resolvable, at least theoretically, into psychological concepts. But just as little as the most accurate and complete mastery of physics and chemistry enables us to synthesize a science of geology, does an equivalent mastery of the conceptual science of psychology—which, by the way, nobody possesses or is likely to possess for a long time to come—enable us to synthesize the actual nature and development of social institutions or other historical data. These must be directly experienced and, as already pointed out, selected from the endless mass of human phenomena according to a principle of values. Historical science thus differs from natural science, either wholly or as regards relative emphasis, in its adherence to the real world of phenomena, not, like the latter, to the simplified and abstract world of ideal concepts. It strives to value the unique or individual, not the universal. "Individual" may naturally here mean any directly experienced entity or group of entities—the earth, France, the French language, the French Republic, the romantic movement in literature, Victo[illegible], the Iroquois Indians, some specific Iroquois clan, all Iroquois c[illegible] all American Indian clans, all clans of primitive peoples. None of these terms, *as such*, has any relevancy in a purely conceptual world, whether organic or inorganic, physical or psychic. Properly speaking, "history" includes far more than what we ordinarily call historical or social science. The latter is merely the "historical" (in our wider sense), not conceptual, treatment of certain selected aspects of the psychic world of man.

Are not, then, such concepts as a clan, a language, a priesthood comparable in lack of individual connotation to the ideal concepts of natural science? Are not the laws applicable to these historical concepts as conceptually valid as those of natural science? Logically it is perhaps difficult, if not impossible, to make a distinction, as the same mental processes of observation, classification, inference, generalization, and so on, are brought into play. Philosophically, however, I believe the two types of concepts are utterly distinct. The social concepts are convenient summaries of a strictly limited range of phenomena, each element of which has real value. Relatively to the concept "clan" a particular clan of a specific Indian tribe has undeniable value as an historical entity. Relatively to the concept "crystal" a particular ruby in the jeweler's shop has no relevancy except by way of illustration. It has no intrinsic scientific value. Were all crystals existent at this moment suddenly disintegrated, the science of crystallography would still be valid, provided the physical and chemical forces that make possible the growth of another crop of crystals remain in the world. Were all clans now existent annihilated, it is highly debatable, to say the least, whether the science of sociology, in so far as it occupied itself with clans, would have prognostic value. The difference between the two groups of concepts becomes particularly clear if we consider negative instances. If, out of one hundred clans, ninety-nine obeyed a certain sociological "law," we would justly flatter ourselves with having made a particularly neat and sweeping generalization; our "law" would have validity, even if we never succeeded in "explaining" the one exception. But if, out of one million selected experiments intended to test a physical law, 999,999 corroborated the law and one persistently refused to do so, after all disturbing factors had been eliminated, we would be driven to seek a new formulation of our law. There is something deeper involved here than relative accuracy. The social "law" is an abbreviation or formula for a finite number of evaluated phenomena, and rarely more than an approximately accurate formula at that; the natural "law" is a universally valid formulation of a regular sequence observable in an indefinitely large number of phenomena selected at random. With the multiplication of instances social "laws" become more and more blurred in outline, natural "laws" more and more rigid. However, the clarification of the sphere and concepts of social science in its more generalized aspects is a difficult problem that we can not fully discuss here.[1]

I strongly suspect that Dr. Kroeber will not find me to differ essentially from him in my conception of history. What I should like to emphasize, however, is that it is perfectly possible to hold this view of history without invoking the aid of a "superorganic." Moreover, had the uniqueness of historical phenomena been as consistently clear to him as he himself would require, it would be difficult to understand why he should have insisted on eliminating the individual in the narrow sense of the word. E. SAPIR.

GEOLOGICAL SURVEY,
OTTAWA, CANADA.

THE AUTONOMY OF THE SOCIAL.

IN a stimulating disquisition on "The Superorganic" (this journal, April–June, 1917, pp. 163–214) Dr. Kroeber once more returns to the topic which of late he has made peculiarly his own.

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[1] For a penetrating analysis of the fundamental distinction between historical and natural science I strongly urge all anthropologists, and social scientists generally, who are interested in method to refer to H. Rickert's difficult but masterly book on *Die Grenzen der naturwissenschaftlichen Begriffsbildung; eine Einleitung in die historischen Wissenschaften.* I have been greatly indebted to it.

With the major, critical, destructive part of the essay I must express unequivocal agreement. With Dr. Kroeber I hold that; (1) the biological point of view fails to account for social phenomena (autonomy of history with reference to race and heredity); (2) the psychological point of view fails in the same connection (autonomy of history with reference to general psychology); (3) the problem of individual ability is also irrelevant (autonomy of history with reference to individualities).

But my agreement with Dr. Kroeber stops short of his formulation of the concept of historic autonomy. Having rejected alternative interpretations, Dr. Kroeber voices his belief in cultural or civilizational determinism. Events in history occur when they must occur. As evidence, the author adduces the many instances in which similar, almost identical, inventions or discoveries were made contemporaneously and independently by two or more men. Here we must call for a more precise determination of the group the civilization of which is being considered. Is it the civilization of the world at large? If that were the case, then, of course, there would be truth in the proposition: What is must be, all that is is reasonable, in the sense of having a reason, a cause. Unless indeed we are prepared to admit that one or another part of the cosmic whole may suddenly run amuck and play havoc with law and order, such a universal determinism must, of course, be accepted as a conceptual necessity, with reference to civilization, or anything else. But in the solution of concrete historic problems such a deterministic faith, while basic, is helpful not at all. I believe, however, that Dr. Kroeber is not speaking of civilization but of civilizations. Now, with reference to any particular civilization the deterministic creed breaks down. Mass phenomena and the theory of probability will, of course, within their domain, continue to apply. An invention being a function of knowledge and effort, when the requisite state of knowledge is reached and a number of minds are concentrating on a certain problem, the probability of the invention or discovery being made by one or more minds is high, and if the number of minds is large, the probability will approach certainty. On the other hand, the same invention may be made at a time when the probability of that event is very low, owing to the presence of an exceptional mind or through importation from another civilization. An invention thus made may have the same or greater historic consequences than would be the case in the alternative instance. Now in this as in a thousand and one similar instances the events may not be regarded as civilizationally or socially predetermined; with reference to the culture of the group they are accidental. The theory of probability breaks down here, the individual event alone counts; and it may make the difference, for all time, between a high and a low civilization.

My second point of difference with Dr. Kroeber refers to the relation of a civilization to the chain of individuals who are its carriers. I grant that the civilizational stream may be considered as a closed system and that much can be gained from studying it as such. Dr. Kroeber, in his turn, grants that useful knowledge, even about the stream of civilization, may be gleaned from a study of its component individuals. But this is not going far enough. As a matter of fact, the civilizational stream is not merely carried but is also unrelentingly fed by its component individuals. The term "individual," in this context, requires specification. It is not the biological individual, nor is it the abstract being of general psychology, nor is it this or that more or less gifted individual; it is not even the average individual who partakes of the given civilization. The individual who counts here may be designated as the biographical individual. He is a historic complex *sui generis*. Neither *biological* nor psychological, nor civilizational factors exhaust his content. He has partaken of the culture of his social environment, but only of certain aspects of it, and these have come to him in a certain individual order, at certain definite places and times, and have been, on all these occasions, received and absorbed by a psyche that was unique. This is the concrete individual of historic society. He is unique and as such he reacts on the civilization of which he is the carrier. Leave him out, and a blind spot appears in the record of civilization. I am convinced that not along the objective study of civilization lies that "path of merely but deeply understanding phenomena," to which the author refers at another place. The inferiority of our knowledge of Kwakiutl or Yoruba civilization when compared to our knowledge of the civilization of France or Germany is in part due to the inferiority of the civilizational record; but it is also due, perhaps more significantly, to the vast inferiority of the biographical record. This is the crying need the modern ethnologist tries to supply; and to the extent to which he is successful, our knowledge of primitive civilization becomes more deep. A. A. GOLDENWEISER.

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THE STATUS OF WASHO

MR. JOHN P. HARRINGTON'S announcement of the genetic relationship of Washo and Chumash (*American Anthropologist* (N.S.), 1917, p. 154) is welcome confirmation of a result which the undersigned had independently arrived at, namely, the necessity of including Washo in the Hokan group established by Drs. Dixon and Kroeber. Specific Chumash-Washo resemblances had also been noted by the writer. That Chumash is a member of the Hokan group had been suspected by Drs. Dixon and Kroeber, further corroborated by incidental data advanced by the writer of this note in linguistic papers just published or soon to be published, and clinched by Mr. Harrington's previous announcement of the clear genetic relationship of Chumash and Yuman, a typical Hokan group of dialects. As for Washo, the writer has already gathered a quite considerable mass of lexical, phonologic, and morphologic data that, at least in his opinion, conclusively establish the Hokan character of this language. In grammatical respects, indeed, Washo would seem to be more typical of Hokan than Yana, which clearly belongs to the group (a paper on the status of Yana has recently appeared in the *University of California Publications in American Archaeology and Ethnology*). The writer hopes to reduce his Washo data to the form of a systematic paper before long. It is highly gratifying to note that far-reaching reclassifications of American languages are being independently and corroboratingly arrived at by students of American linguistics. Evidently the cautions that have been urged by certain more conservative students are not, despite their methodological excellence, exercising an unduly deterrent influence.

The Hokan stock can at present be stated to include Shasta-Achomawi, Chimariko, Karok, Pomo, Yana, Esselen, Salinan, Chumash, Yuman, Washo, Seri, and Chontal. A paper of the writer's, written some time ago, but publication of which has been delayed, undertakes to demonstrate the genetic relationship of the Hokan languages to the Western Gulf or Coahuiltecan group (Coahuilteco, Comecrudo, Cotoname, Karankawa, Tonkawa, possibly also Attacapan) recently set up by Dr. Swanton. It is very interesting to note that the territory separating Coahuilteco-Karankawa-Tonkawa from Yuman-Chumash-Washo is almost entirely occupied by Shoshonean and Athabaskan tribes, the latter of which are beyond all reasonable doubt an intrusive element from the north, while the former are only less certainly representatives of a relatively recent northward and eastward spread of Uto-Aztekan tribes from, say, southern Arizona and northern Mexico. The elimination of Washo leaves Yuki as the only Californian language that can be called "isolated." It is hardly to be expected that this privilege can be allowed Yuki indefinitely.

E. SAPIR

GEOLOGICAL SURVEY,
OTTAWA, CANADA

PROCEEDINGS OF THE AMERICAN ETHNOLOGICAL SOCIETY, INC.

Meeting of February 28, 1917

THE Society held a Tylor Memorial Meeting in honor of Sir Edward Burnett Tylor (d. Jan. 2, 1917). Dr. Goddard was in the chair, and the following members were present: Mrs. Donohugh, Dr. Fishberg, Dr. Frachtenberg, Dr. Goldenweiser, Dr. Haeberlin, Dr. Lowie, Miss Mallory, Miss Mayer, Mr. Mead, Mr. Morris, Mr. Nelson, Mrs. Parsons, Miss Roberts, Mr. Schleiter, Mr. Sullivan, Mr. Spier, Dr. Spinden, Dr. Wissler.

In the absence of Professor Boas, who was to have spoken on "Tylor's Influence," Dr. Wissler made some introductory remarks on the significance of Tylor in the history of anthropology and as a protagonist of evolutionary principles. From this he turned to the special topic of his discourse, "Tylor and Material Culture." While this phase of Tylor's activity seems relatively unimportant as compared with his other contributions, it must not be underestimated. Thus, it was he who blocked out the essential processes of primitive fire-making in his *Researches into the Early History of Mankind*, the later work of others being hardly more than an amplification, especially as regards facts of distribution, of his pioneer work. His discussion of cooking was equally important, and in the chapter on the Stone Age he clearly developed the point that the stone age represents rather a state of culture than a chronological epoch.

Dr. Lowie spoke next on "Tylor on Diffusion and Adhesions." While Tylor is generally associated with evolutionary schemes, it is interesting to find that he went rather far in explaining similarities by diffusion and in fact went farther in specific instances than most ethnologists would be inclined to go today, *e. g.*, in his discussion of the cure of sick people by sucking out the pathogenic agent. His treatment of the Malagasy bellows is a noteworthy instance of a positive contribution to ethnology by use of the principle of historical connection. The method of adhesions proposed by Tylor in 1888 and sketched the following year in the *Journal of the Royal Anthropological Institute* involved the application of a rigorous statistical treatment to ethnographic data. When two phenomena were found in conjunction with a certain frequency, Tylor inquired whether this frequency was to be expected on the laws of probability, and if it was decidedly greater he assumed a causal relation between the two. The basic weakness of his discussion, as pointed out by Galton, lay in the neglect of diffusion: Tylor failed to consider in how far the several instances of correlation found might have spread from a single center. When, however, due attention is paid to the historical distinctness of the areas concerned, his method becomes an exceedingly valuable tool for ethnological research.

Dr. Goldenweiser then spoke on "The Doctrine of Animism." He explained Tylor's minimum definition of religion as the belief in spirits and pointed out that Tylor's treatment of the subject was rather a contribution to comparative religion than merely an evolutionary scheme. The speaker contrasted it favorably with Herbert Spencer's corresponding theory and again called attention to Tylor's recognition of the diffusion principle. Recent criticisms passed on Tylor's theory of animism come from several directions. The sociological school of France criticized the representation of religious phenomena from the viewpoint of individual psychology. Lang emphasized abnormal psychological phenomena over and above the normal ones on which Tylor laid the main stress. Further, Lang agreed with Father Schmidt in urging that primitive man already had a belief in a supreme otiose high-god. Again, Marett and others have insisted on the importance of the religious thrill, which is neglected in the more rationalistic view attributed to savage believers in spirits. Nevertheless, in its essential features Tylor's doctrine of animism still stands in spite of all modifications and amplifications of detail.

The president called on those present to open the discussion. Mr. Spier asked Dr. Lowie to explain how Tylor's method was applicable where the units were less clearly defined than, say, in the case of the mother-in-law taboo. Dr. Lowie replied that it was not so important whether the elements descriptively separated were historically distinct. In either case the establishment of a nexus would prove enlightening.

Dr. Frachtenberg expressed his appreciation of Tylor's religious theory and the confirmation he had found of it in the field among Northwestern tribes.

Dr. Lowie called attention to the fact that Tylor had had some field experience in Mexico in his early manhood and asked Dr. Spinden to make some remarks on Tylor's book *Anahuac*, now rare and little known in America.

Dr. Spinden explained that Tylor's work was a precocious effort still referred to for its descriptions of Mexican antiquities in British collections. He added some remarks expressing the admiration experienced when first becoming acquainted with Tylor's *Primitive Culture.*

Dr. Goddard called attention to Tylor's promotion of the important field work in British Columbia undertaken by Dr. Boas for the British Association for the Advancement of Science and the consequent indebtedness to Tylor of all American students of ethnology. He suggested the publication of the addresses delivered this evening, as well as of Professor Boas' scheduled remarks.

The meeting adjourned at 10 p. m.

Meeting of March 26, 1917

The meeting was called to order by Dr. R. H. Lowie, but Dr. Goddard, who had been delayed, took the chair on his arrival.

Mr. Herbert Lang, the African explorer, spoke first on "Dances of the Natives of the Belgian Congo." The tribes, whose dances were shown in a series of colored slides, are all resident in the northeastern Congo basin, covering an area which extends from the equator to nearly 10° north and from 1,200 to 2,000 miles inland. They are the Mangbetu, Medje, Makere, Abarambo, Azande, Logo and also the Pygmies. Their present distribution was explained by means of a map and remarks were made on the general influence of the physiographical conditions—especially the flora—on the population in respect to their social organization and their dances.

The dances considered may be divided into three groups: (1) those of a merely social order, *i. e.*, for amusement or the propagation of good fellowship; (2) ceremonial dances, which show great variety but are wholly adapted to circumstances; (3) the dances of a rather religious aspect for the propitiation of the bad spirits, for the sake of divination or similar reasons.

Slides illustrating the environment, the villages, and a few types of men and women led up to accounts of the usual preparations preceding each particular dance. Short discussions were presented of their costumes, hairdresses, the decorative designs on their body, and some of the musical instruments used, as well as the intoxicants that often provide the necessary feeling of gaiety or excitement.

The dances shown furnish an important contribution toward the better understanding of the psychology of these negroes. Immorality is practically barred and by far the greater number of all the dances are joyous demonstrations in which personal ability and individuality have an opportunity to gain admiration or render themselves useful to the community. The craze for sensation is demonstrated in many ways by the costumes, as well as by the peculiarities of the dances. But foremost is the fundamental knowledge that, through easy abandonment to social enjoyments, they are able to submerge the trivialities and hardships of the daily life.

Dr. Elsie Clews Parsons next presented a paper on "The Office of Governor at Zuñi."

There are at Zuñi a governor (*tapup*) a so-called lieutenant governor (*sipoloa shiwanni*, Mexican rain-priest) and nine *tenientes*, a board of eleven only recently enlarged from eight. These officers are chosen by the six head *ashiwanni* (rain-priests). They hold office at the pleasure of the *ashiwanni;* but when they are to be relieved the change is made either at the Saint's dance in September or some time about January first. This year the *tenientes* who had been in office for five years were relieved and on January 16 new officers were installed. The ceremony, which consisted of passing the canes of office from outgoing to incoming officers was held in the house of the head rain-priest. It was managed by the head bow-priest. It was characterized by Mexican (Spanish) features—genuflexion and the sign of the cross. The use of physical force as a sanction, as mentioned in the governor's prayer, may also point to Spanish influence. The texts of the prayers said by the outgoing governor and the head rain-priest were subsequently secured and translated. They are the first prayers recorded, as far as I know, in Zuñi. The prominence of the *tenientes* at the celebration of the Saint's dance (they sit at the altar) is another indication of the Spanish character of their office. Spanish or not, the institution is thoroughly at home. Its undoubtedly Zuñi features are the long-time customary number of the *tenientes*, eight as a multiple of four being a favored Zuñi numeral, the control of the *tenientes* by the priesthood, the rite of breathing on the cane of office at the installation (the word *tapup* means breather on the cane), the fetichistic character of the cane (unless a *teniente* were in actual possession of his cane, his authority would be questioned), the obligation upon the *tenientes* to present a case or issue an order *four* times, and in some cases the crying out of orders *four* days in advance.

At Acoma corroboratory evidence of the Spanish origin of the institution was secured. At Acoma the governor (*tapup*) and two *tenientes* are chosen annually by the *cacique*, the head of the Antelope clan, the ruling clan. The installation occurs December 30, at the conclusion of a ceremonial week of church-going and of dancing. The installation occurs in an assembly building called *komanira*. It is a house near the church and it is of Spanish rather than Pueblo architecture. In it the governor holds court. At the installation, genuflexion occurs as well as the removal of hats and the sign of the cross with the "Mexican prayer."

Some time after the installation (this year January 27 and when I was at Acoma) the *tenientes* and the three "war chiefs" elected with them give a "dance," the *hoinawe*. It corresponds to the *owinahaiye* dance of Zuñi, a dance which was associated in Zuñi with the bow priests, and which has not been danced for six or seven years. Firing off guns, a Spanish feature, used to figure in the Acoma dance as in the Zuñi. Throwing presents of food, etc., either to the dancers or by the dancers is still a feature in the *hoinawe* as it was in the *owinahaiye*. (At Zuñi *largess* seemed to have been originally confined to the *owinahaiye*, to the other bow-priest ceremonial, the scalp dance, and to the Saint's dance.) Largess in this form may have been introduced by the Spanish. The older custom in Zuñi was house-to-house collecting or sending out food to the ceremonial house.

Both at Zuñi and Acoma the bow priests or war-chiefs figure conspicuously, not only in the dances referred to, but in connection with the installation of *tenientes*. It is suggested that in pre-Spanish days the war chiefs (or bow-priests) were the secular arm of the priesthood, representing the kingly rather than the priestly power. With the advent of the Spaniards and the imposition by them of the type of government familiar to them and congenial, the war-chiefs tended to be deprived of their more secular functions and to be thrown back on their more sacerdotal functions. (At Laguna where the rain priesthood is disintegrated it is the war-chiefs who perform the monthly prayer plume plantings for rain and social welfare.) At the same time there grew up a tendency, more among the Keres than among the Zuñi, to connect the war-chiefs with the *tenientes*.

This hypothesis would help account for the influence of the war officers at Zuñi in selecting *tenientes*, for their function as masters of ceremony at the installation of *tenientes* alike at Acoma and at Zuñi, and for the performance of the *hoinawe* at Acoma to celebrate the *festa del re*.

At Acoma the war-chiefs became more assimilated with the *tenientes*, at Zuñi with the priesthood. At Acoma (and Laguna) the war-chiefs have survived through their assimilation with the *tenientes*. For lack of that assimilation at Zuñi there is a fair prospect of their disappearance.

It is an illustration of how the life of an institution may depend on being adapted to new circumstances.

The paper was discussed by Dr. Lowie, Mr. Morris and Mr. Spier.

Meeting of April 30, 1917

THE meeting was called to order at 8.25 p. m. Dr. P. E. Goddard presided. He welcomed the lecturer of the evening, Professor S. A. Smith, of the University of Sydney, who presented a paper, illustrated with slides, photographs, diagrams and a cast, on "The Talgai Skull; a Fossil Human Skull found in Queensland, Australia."

The lecturer explained that the skull had been found some thirty years ago, but had only recently been examined by scientific men. His discussion was necessarily a purely anatomical one. The skull belonged to a boy from fourteen to sixteen years of age. Its facial characteristics and general cranial aspect do not differ from those of normal Australian skulls, but the palate and dentition are of an exceedingly primitive character and on this may be based an argument for the very respectable antiquity of the Talgai man.

The paper was discussed by Dr. Wm. K. Gregory, Dr. J. H. McGregor, Dr. Milo Hellman, Mr. Louis R. Sullivan and Mr. Ralph W. Linton.

ROBERT H. LOWIE
Secretary

ANTHROPOLOGICAL NOTES.

Dr. John R. Swanton, of the Bureau of American Ethnology, returned to Washington in the latter part of June from an expedition to Louisiana, covering six weeks, during which he visited the remnants of the Houma, Chitimacha, and Koasati Indians. Some ethnological and linguistic notes were obtained from the two first mentioned, and a great many pages of texts secured from the Koasati. Koasati, although closely related to Alabama, differs from it somewhat more than had been supposed.

Mr. J. N. B. Hewitt, of the Bureau of American Ethnology, has returned to Washington after a field trip covering four months and devoted mainly to researches concerning the origin and structure of the League of the Iroquois. He spent most of his time on the Grand River Reservation, Ontario, but was present at the condolence and installation ceremony of the League which took place at Muncie, during the period of his visit.

Dr. Truman Michelson, of the Bureau of American Ethnology, left Washington on July 11, to resume his studies of the Algonquian tribes of Iowa, Kansas, and Oklahoma.

Dr. Aleš Hrdlička, curator of physical anthropology, U. S. National Museum, left Tuesday, July 31, for a trip among the Tennessee mountaineers, in connection with his work on old Americans. Dr. Hrdlička will be accompanied by Mr. King of Bristol, Tennessee, who knows the region and is acquainted among the mountaineers. From there Dr. Hrdlička will go among the Shawnee Indians of Oklahoma to examine the full-bloods still existing in that tribe, with a view of finally deciding the question as to whether the Shawnee are descendants of the important "stone grave" people of Kentucky and Tennessee.

Dr. Walter Hough, of the U. S. National Museum, continued work during the month of June on the ancient pit-dweller village at Luna, New Mexico, collecting additional artifacts and information. Another site containing fifteen acres was discovered seven miles north of the Luna site. A report on this work is in course of preparation. He also made observations on the ceremonial mask-headdress of the White Mountain Apache of Arizona, and collected a number of interesting specimens of paraphernalia.

Mr. Neil M. Judd, of the U. S. National Museum, was engaged during the Spring in effecting repairs on Batatakin ruin, Navajo National Monument, northeastern Arizona, for the Department of the Interior. Subsequently he took charge of archaeological work in Utah, which is carried on in cooperation by the Smithsonian Institution and the University of Utah.

THE excavation of the Aztec ruin in New Mexico, begun by the American Museum of Natural History last summer, is being continued on an extensive scale under the immediate direction of Earl H. Morris. The work has, so far, yielded a very fair return in specimens of unusual interest, particularly pottery and textiles.

Mr. N. C. Nelson left New York August 13, to round out the archaeological work in which he has been engaged for the past five or six seasons. This work had for its center the region of the Galisteo valley, but has spread to considerable distances in other directions.

THE degree of Doctor of Philosophy has been given by Harvard University to William H. Mechling, Carl E. Guthe, and N. Utsurikawa. The subjects of their theses were, respectively, *The Malecites and the Micmacs*, *The Luna Count of the Mayas*, and *The Art of the Malay Peninsula*. Mr. Mechling has received an appointment on the staff of the Field Museum and Mr. Guthe in the Department of Archaeology at Andover. Mr. Utsurikawa has received a Sheldon Traveling Fellowship and is to continue his studies in Malaysia.

During the temporary cessation of work at the Pecos ruin, Dr. Guthe of the Andover expedition staff, is carrying on explorations to the east of Pecos and in the northern Rio Grande, in an endeavor to define the eastern and northern limits of Pueblo culture in New Mexico and Colorado.

THE Natural History Collections of the Peabody Museum of Yale University, including the Anthropological Collections, have for the most part been stored pending the erection of a new museum building. Meanwhile a few of the anthropological exhibits, including the Moseley collection of Indian basketry and the Prudden collection of antiquities from the Southwest have been installed in Memorial Hall.

WILLIAM WALLACE TOOKER, an authority on Indian nomenclature and archaeology, died on August 1, after a long illness at his home in Sag Harbor, L. I., at the age of sixty-nine years.—*Science*

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SOME ANTHROPOLOGICAL MISCONCEPTIONS[1]

By JOHN R. SWANTON

STUDENTS of human culture are aware that all cultural phenomena pass through a series of changes, from their inception to their general acceptance or vogue, then to their decline and finally to their extinction or loss in a mass of other phenomena. As history reveals no movement which has failed to repeat this sequence the need of constant reformation, revival, recreation, origination is apparent, and constant guard against institutionalization, corruption, ossification. This is no less true of science than of religious beliefs, of art, of social institutions; it is no less true of anthropology than of any of the older sciences. The continued vitality of anthropology depends on repeated renewal. And when I say this I do not mean the perpetual addition of new facts to our store but new deductions from those facts, new principles revealed through them, new hypotheses as tentative efforts toward the establishment of these principles. Properly speaking there is, or should be, no such thing as orthodoxy in science. Science is in danger unless she is perpetually heterodox. All that we have a right to demand is that each new principle or theory shall be based upon a sufficient study of sufficient data, and that the method pursued be sound.

In science as in other movements of the human mind we have seen new eras of thought opened by certain great ideas, we have seen these ideas spread, and we have seen them gradually fade away. But we must also remember that what was true in the past is also true of the present. Too many students, while clearly understanding the processes of intellectual birth, growth, and death as applied to previous ages vainly imagine that they themselves live in the era of fulfilment, that the old cycle is completed with them, and that the favorite principles of their age are the last pronouncement of truth itself, the perfect knowledge.

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[1] Retiring address as President of the Anthropological Society of Washington, May 1, 1917.

The thought of man in the age in which we are living is dominated both on the scientific and the philosophical side by the doctrine of evolution and powerfully influenced by the principles which Darwin associated with that doctrine, "the struggle for existence," "natural selection," "the survival of the fittest." Of course the growth and expansion of this doctrine, the *essential* truth of which no scientist, of course, doubts, has been marked by a huge amount of discussion, and supplementary hypotheses such as "Neo-Lamarckism," and "mutation" to say nothing of the Mendelian laws, have made their appearance to interest—and also to confound—the student of the subject. Judging by the past, however, we may be sure that whatever is stable in evolution will in course of time be assimilated into the thought of mankind, while the chaff will disappear and presently the world will be ready for some new intellectual upheaval, perhaps more striking still.

But, as is always the case in such movements, the original idea in the course of time has tended to certain misconstructions and has been used in an unwarrantable manner and in situations where it is entirely out of place. While this is true in its original and proper field, biology, the treatment of such perversions is the duty of the professional biologist and I shall now turn to a consideration of the theory as applied to anthropological data.

The application of the evolutionary theory to studies in physical anthropology was of course easy and natural, physical anthropology being to all intents and purposes a chapter of biology itself. Its introduction into philology we owe to those early masters who determined and ferreted out the relationships between the different branches of Indo-European speech. Of course this work antedated in considerable measure that of Darwin himself, but it was impelled forward and the later philological theories largely propounded under stimulation from the evolutionary movement.

The new viewpoint was introduced into investigations of the culture history of mankind mainly through the labors of two distinguished writers, Herbert Spencer and the late E. B. Tylor. Spencer's treatment of human culture formed, however, only the later chapters of his great evolutionary philosophy, while Tylor's interest was and remained almost entirely anthropological.

As this method was introduced from biology it was naturally enough colored by biological conditions. And as in biology we had a sequence of living forms ascending from the single-celled animals, through multiple-celled invertebrates, vertebrates, and mammals to man, and geological evidence that these had appeared on the earth successively beginning with the simpler, it was naturally assumed that among the races of mankind we should find the same conditions, that is, certain "more primitive" races, others less primitive, and others still less primitive, until we came to those peoples who were the custodians—or the victims—of European civilization. Such a view was indeed natural and also justifiable as a general working hypothesis, but its application was accompanied by a strangely one-sided view and a blindness to other plain teachings of biology quite astonishing. For one of the plainest lessons regarding biological evolution is that among the species of a genus or the varieties of a species the most generalized are those which probably stand nearest to the type from which all are descended. Instead of allowing themselves to be guided by this fact our anthropological predecessors took the ground that that was highest developed which approached nearest to the standards of European civilization, that to which they themselves belonged, and that most primitive which diverged farthest from it. Thus western civilization existed among peoples prevailingly fair, therefore the most primitive races must be dark. Other features were also deemed to be "primitive" in proportion to their divergence from European standards. Still stranger results were produced by applying the same standard to cultural features. It was confidently asserted that primitive man was a cannibal, that he tattooed, deformed his head, pierced his nose and ears, was promiscuous in his sexual relations, was divided into totemic clans or rather "hordes," and practised a multitude of other customs because these particular customs and usages are the ones least prominent in the west European cultural center. The idea was carried so far that theories of origin were based upon cultural elements found in one and the same tribe with other cultural elements, but selected as original because they were the ones strangest to a European. Naturally, however, as primitive peoples possessed numerous usages, customs, and beliefs of the same genus differing widely from our own, differences of opinion arose as to the most primitive among these and the order of their development, and in course of time the theorist was ready to seize upon any custom, however obscure, as the first step in an evolutionary series which he was able to elaborate at least as successfully as every other theorist. This method of approach is what Professor W. I. Thomas in his *Source Book for Social Origins* calls "the particularistic explanation of social change" and he gives some amusing examples of the extremes to which it has been carried. He mentions as

> two of Hebert Spencer's great and gross errors of this character—the derivation of all the learned and artistic occupations (even that of the dancer) from the medicine-man and the assumption that ghost-worship is the origin of all spirit belief and worship (even of the worship of animals and plants).

One of the assistant sources of error is an over-earnest attempt to find in cultural elements not thoroughly understood "survivals" analogous to the vestigial characters which play such a great part in upholding the doctrine of biological evolution. But while we can not doubt that there are such things as "survivals" the investigator must first be certain that a cultural element is really such, and secondly he must assure himself that it actually does point back to just the condition which he assumes. For instance, the fact that a symbolic human sacrifice is made by a certain tribe, while suggesting that there formerly did exist a real human sacrifice, is not proof of that fact. It is quite conceivable that it is only a stage play such as is found in many rituals, or that it is a symbolic representation which would ultimately result in human sacrifice but has not yet reached that point. In short the survival theory has been so grossly abused that every supposed case of the kind ought to be examined with scrupulous care.

On account of this selection of the unusual and the strange it has happened unfortunately that the science of anthropology has been turned upside down. Its isolated peaks have been made the bases and the broad features which human society shares in common have been developed from these. The whole must be turned about. We must begin from that which is general and perennially recurring and from it work out all of the peculiar developments. At least the assumption should be that the general is first, the peculiar later, and the burden of proof is on him who would maintain the contrary proposition.

In order to illustrate the above point more concretely I will cite some examples of particularistic theories. First I will consider the social phenomena which have been called totemism. When European explorers first came among some of the so-called "lower races" they found what to them was a curious custom. They found these divided into certain septs or clans, each bearing a name referring to a species of animal, plant, or some other object, the members of which considered themselves parts of one large family inside of which they refused to marry. Often the animal from which the clan was named was held in peculiar regard by the members of that clan and certain observances or taboos were in vogue in regard to it. Frequently the personal names of individuals of the clan had reference to the totem animal. These features were found in so many different parts of the world and were so strange to their discoverers that our neo-evolutionist anthropologist jumped to the conclusion that we had here a really primitive feature, that in antiquity all peoples had such a social system, and that where we had anything different it was the result of later changes. And as descent in such tribes was often through the mother and that was again un-European it was assumed that the really primitive society was matriarchal. Attempted explanations of "totemism" at once became the order of the day and along with them various ingenious theories to account for the evolution of society out of that condition. More extensive studies of the actual data served, however, to demolish this castle of cards completely. It then became apparent that all primitive peoples did not have totemism in the orthodox form, in many cases in no form at all, and that the elements entering into it were combined in numerous different ways or else were uncombined. The areas themselves proved not to be coterminous and, while totemic tribes ought, to carry out the theory, to be lowest in culture, those in North America at least were found among the highest. Dr. Goldenweiser's illuminating discussion of the whole question showed clearly that what had been called "totemism" was a complex association of elements existing elsewhere independently and may be said to have put an end to the whole theory of an evolution out of a primitive "totemism."

Frequently associated with this was another favorite particularistic theory explaining the evolution of human marriage. It was found that marriage customs among certain races were very divergent from the standard and legal monogamy, or rather monogyny, of Europe. With polygamy students were already familiar but here and there the converse condition of polyandry was found, and in still others marital privileges extended so far that there appeared to be an actual marriage of a group of men to a group of women. Here once more the strange form was seized upon as the original and a most clever theory of the evolution of human marriage devised. It was held that the anomalous marriages observed pointed to a period when intercourse between the sexes was absolutely promiscuous, that this had been succeeded by marriages between groups, this by polyandry, by polygamy, and finally by monogamy. As in so many cases of the kind one can not withhold admiration for the cleverness with which the theory was propounded and worked out, but when the basal facts upon which it rested were critically examined only another house of cards was revealed. It had to be admitted that the stage of absolute promiscuity exists nowhere today and must remain purely hypothetical, that the cases of so-called group marriage are ridiculously few to form a base for such a structure, and that polyandry and polygamy existed side by side with monogamy, were largely to be explained by economic and social conditions, and could not be shown to be older than the monogamy which they accompanied. Finally it was observed that monogamy had already appeared in the animal series and something closely resembling it was found in our nearest anthropoid relatives. The conclusion has therefore been reached that monogamy has probably always been the normal form of human marriage, and that other forms are aberrations from this norm.

Perhaps one of the most typical as well as one of the most ingenious of particularistic theories is Spencer's explanation of the origin of religious belief. Originally, he held, man had no religion. But in his dreams he seemed to see as still living persons he knew to be dead. Accepting his dreams at their face value he therefore supposed that those who had died were still living, though in a different state from that with which they had formerly been associated. From this deduction came the belief in another world, a cult of the dead, and a reverence for those places where the dead were buried. Now it might have happened that the body of some chief was laid away upon the top of a mountain. His soul would then be supposed to be associated with that mountain, and when there was thunder and lightning there the phenomenon would be attributed to him. In this way he would become associated with celestial phenomena, and in course of time, his apotheosis having been forgotten, he would be reverenced as a sky god. In equally ingenious ways he shows how belief in spirits in the other phenomena of nature would arise from a simple belief in the continued life of ancestors. From the polytheism thence resulting, monotheism arose through the progressive reduction of the number of beings to whom reverence was paid. Tylor's chain of evolution is much shorter and far safer. He merely assumes that the spirit world was a deduction from such natural phenomena as dreams, reflections in water, shadows, the invisible yet sensible wind, and points for confirmation to the names for soul, spirit, and so on, which are frequently identical with or deduced from these. Frazer, attacking the question from a different angle, picks out magical practices as his point of departure and assumes that the belief in spirits or gods was subsequently introduced.

All of these theories are, it will be seen, particularistic. Each selects one particular feature from the mass of phenomena and arranges the rest in a series ending with the dominant belief of civilized men. As in the other cases, some element of belief particularly strange to so-called "civilized" people is selected to start the series, and each chain of evolution leads dutifully up to either the monotheism or the atheism of western Europe.

As in the other cases our answer to these theories is that the selection of one feature rather than another lacks validity, and that the arrangement of the evolutionary steps is arbitrary. Furthermore, instead of being scattered through different peoples which might thus be considered to represent so many distinct stages in the evolution of religion one or more of these elements are frequently found in the same tribe and all of them are found in tribes equally primitive. On the other hand most of them flourish among tribes which can hardly be considered primitive. The Haida Indians of British Columbia, for instance, might be said to have worshiped ancestors since they sent food and clothing to their dead friends through the medium of fire, they had magical practices, believed in a large number of supernatural beings, and had a belief in a celestial deity recognized as in some measure superior to all the rest. On data drawn from this one tribe each of the above theories of the origin of religion, could be established. In fact the data at hand up to this point suggest that each element thus seized upon as a point of origination has in fact had an independent and parallel history. Even in the case of our regnant monotheism it is a fair question whether it does not tie on to a belief in a sky god extending back to the earliest days of religion among men, the only change which it has undergone being the relatively greater importance and deeper spiritualization of the concept in later times.

A second error has crept into anthropology from an over-accentuation of the uniformitarian side of evolution. In order to combat "special creation" the early champions of evolution naturally strove to minimize differences between species, genera, and so on, but in doing this they somewhat overplayed their hand, and neglected another natural law just as patent as evolution itself. Absolute uniformitarian evolution, regular movement like that of a ball rolling down hill could exist only in a world not discriminated into parts, one without definite atoms or definite individuals, a world in which one thing gradually shaded into another. Our world is not, however, of that kind. If we turn to the physical world we find that it consists of molecules and if we divide these up we have still other definite units, atoms, and that if we divide these up we have yet others, electrons. And if analysis ever goes farther there is every reason to believe that we shall still be confronted by distinct entities. Therefore, such a phenomenon, for instance, as the growth of a plant, while apparently perfectly continuous, is shown to be the successive lifting into place of these units of structure. Turning to biology we find ourselves confronted by individual organisms, each of which has its definite inception, its independent existence, and finally its extinction. Evolution certainly assumes nothing more continuous than the passage of life from parent to child, yet in the sense that the offspring is an independent entity in that sense there is a break at every generation and evolution is not and can not be an absolutely uniform process. And when we ascend higher and inquire into the evolution of human culture this fact is just as striking. It frequently happens that so many successive steps have entered into the evolution of an industry, a social organization or custom, or a ritual, that it is described as if it had been evolved after the uniformitarian pattern. Actually, however, we know that it came about by certain successive steps, each taken at some definite time and place, although we may not be able to say exactly where or when.

Closely associated with the uniformitarian view of evolution in human culture is a common notion that this evolution was unconscious. No one who has spent much time in anthropological investigations will be inclined to minimize the strength or significance of the unconscious or subconscious element in the development of human society, but it may easily be over-emphasized. When things are done without attention the tendency is either to reproduce the habitual action without essential variation or to degeneration. A ritual repeated without a constant mental revival of its meaning will probably change slowly in time, and degenerate, but at the same time, unless the ritual is doomed to disappear *in toto*, other elements will probably be introduced, elements answering better to the living interests of the people at the time when the introduction takes place. The action of the unconscious or subconscious mind is rather towards conservation or degeneration, the positive advances which human society makes are more often through conscious application of the mental faculties. Once a new process, institution, custom, law, or belief has been accepted and become current it passes into the realm of the habitual, that is of the unconscious, and continues to benefit or injure the community in which it has taken root, perhaps for uncounted generations. Moreover it has become a precedent for other actions and beliefs which may be still better or still more harmful. It is therefore of great importance that each change introduced into the collective body be in the line of progress. Each such choice which society makes means the entering upon one road rather than another, it means one set of experiences throughout the entire future rather than another, it means one history for the entire tribe rather than another.

I will not here enter upon that ancient discussion as to whether there is any such thing as freedom of choice. The ablest minds in the philosophic world have threshed this question out in every possible detail and it would be difficult to add anything to the discussion. The weakness of the determinist attitude lies, however, in the fact that, whether the determinist feels he has proved his case or not, he acts immediately afterward as if he had not done so. We must assume the possibility of choice or else our deliberative assemblies whether political, scientific, or philosophical have no meaning whatsoever. Opinion can have no significance for the consistent fatalist.

This being understood I wish to register a protest here against the extreme uniformitarian evolutionist, and the working of the unconscious element in the development of human thought to death. Too many students of history, using this term in its broadest sense, impressed by the gradual nature of change meet new conditions and new problems in the world about them with the cynical remark that *all that* "has been thought of before," that "it has been tried and has failed," that "you can't change human nature," "that you can't hasten the processes of evolution," and that "if it is destined to happen it will happen anyhow." While such an attitude is understandable it is based on a very one-sided reading of human history. Of course it does take time to bring about change, but if we study the advances made by humanity we shall find that they have come not through any extra-mental impulse of the uniformitarian pattern but by progressive choices of individuals and collections of individuals at particular times and particular places. The assumed steady current is found to be directed and determined at every point by individual wills. It is what is called in physics a "resultant of forces" of which those individual wills are the component elements. We must not allow the fact that the accomplishment of each will is but a small increment of the general mass to blind us to the fact that it is after all the determinating factor in the entire process of evolution *on its constructive side.*

And thus, for the progress of humanity and the good of the race it is necessary that the pale student, profound and cynical, learned and skeptical, phlegmatic and pessimistic, be supplemented by the man of action, the reformer, the prophet of the future. Ill informed as many of these men are on the details of the past development of their race they make up for it by an understanding of the psychology of the present, by their keen appreciation of ethical values, their sensitiveness to the better and the worse in the institutions, the processes, the laws and customs, and the beliefs of their time. The equipment of the historian of human culture—the anthropologist in the broad sense—is not to be minimized. It is an important element in determining future action, lack of which may result in unproductive and ill considered change, but its possession guarantees no one an ability to deal with the problems of the present or introduce laws and institutions productive of future welfare for his nation or his race. Where the two types of mind are combined in due proportion we have the highest type of legislative genius.

Bureau of American Ethnology,
Washington, D. C.

CERTAIN ARCHAEOLOGICAL INVESTIGATIONS IN TRINIDAD, BRITISH WEST INDIES[1]

By THEODOOR DE BOOY

Introduction

As this paper is intended solely to put on record the excavations made by the writer and some of the resulting specimens, it does not seem advisable to go extensively into the previous literature existing on the archaeology of Trinidad. Neither does the writer consider it imperative to illustrate many of the artifacts found or to discuss them at length, he being more desirous of explaining the conditions under which the objects were found. Some three thousand specimens in all were sent to the Museum of the American Indian from Trinidad during the four months that the writer worked on this island and it would require a bulky monograph to thoroughly describe each separate specimen.

Dr. J. Walter Fewkes, of the United States Bureau of American Ethnology, made a survey of some of the shell-heaps at Erin Bay on the south coast of Trinidad for the Museum of the American Indian (Heye Foundation) (which at that time was known as the Heye Museum),[2] and the Reverend Thomas Huckerby, who was then living in Trinidad, conducted some excavations on the St. Bernard estate near Cape Mayaro, the results of which excavations are now in the Museum of the American Indian (Heye Foundation) as are the specimens obtained by Dr. Fewkes at Erin Bay. The Huckerby specimens proved to be somewhat different from those collected by Dr. Fewkes, and as Mr. Huckerby obtained only comparatively few objects, the writer decided to make a thorough investigation of the remains on the east coast of Trinidad, to see if a different culture could have existed there. On arrival at Port-of-Spain, therefore, he was introduced to Mr. Paul Urich, who owns extensive plantations on the east coast of the island near Cape Mayaro (see map, fig. 61) and received a cordial invitation to come there and to excavate on the St. Bernard property. It was on this same property that Mr. Huckerby had made a number of somewhat superficial investigations and had collected those specimens that had first drawn the writer's attention to a difference between the east coast and the south coast pottery.

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[1] After an archaeological exploration of the island of Margarita off the north coast of Venezuela from February to May, 1915, described by the writer in a previous paper, (Booy, Theodoor de, "Certain Archeological Notes on the Island of Margarita (Venezuela)," *Contributions from the Museum of the American Indian* (*Heye Foundation*), Vol. II, Number 5), a certain amount of work was done in the British island of Trinidad, 120 miles to the eastward of Margarita and the writer remained here from May until September of the same year. The local government of this latter island and a number of the residents and landowners were of great assistance to the writer in his archaeological work and he wishes to take this opportunity to tender the thanks of both the Museum of the American Indian (Heye Foundation) and of himself to Paul Urich, Esq., of the St. Bernard Estate near Cape Mayaro, on whose property the greater part of the work was done, to Mrs. Paul Urich for her unfailing kindness and hospitality, to Carl Boos, Esq., of Port-of-Spain, for his aid in innumerable matters and to the officials of the Royal Victoria Institute of Port-of-Spain.

[2] Fewkes, J. Walter, "Prehistoric Objects from a Shell-heap at Erin Bay, Trinidad," *American Anthropologist* (N. S.), vol. 16 (April–June, 1914), pages 200–220.

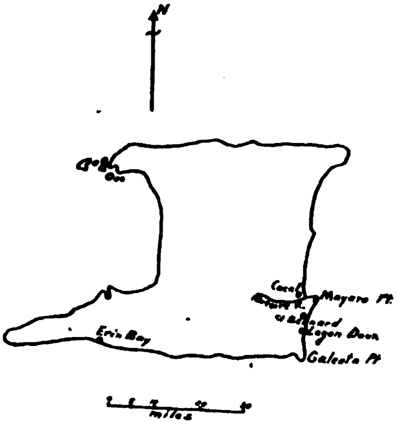
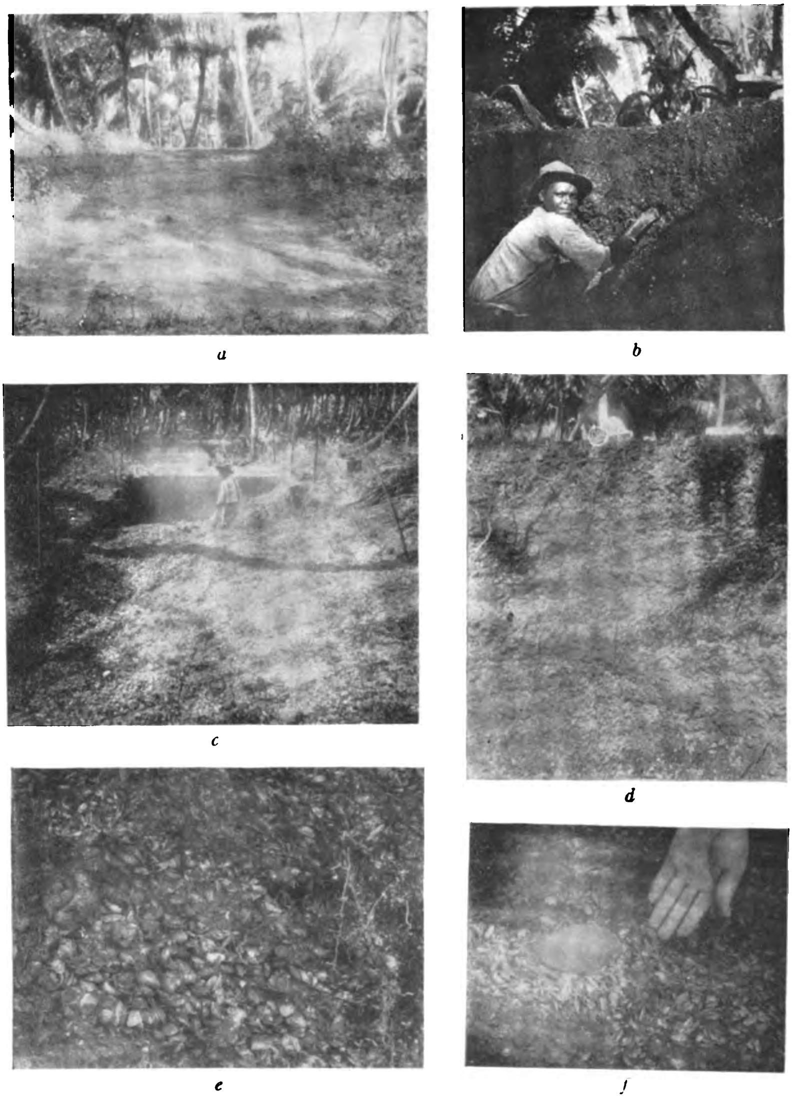


FIG. 61.—Map of Trinidad.

The shell-deposits in which the pottery and other pre-Columbian objects were found are situated on the St. Bernard property, about one and one half miles south of Cape Mayaro and one fourth of a mile inland from the sea beach. It is likely that in pre-Columbian times these deposits were a far greater distance from the sea, possibly from three fourths of a mile to a mile. The tides on this part of the east coast of Trinidad are so strong that the seashore is constantly being pushed further inland; on riding along the shore, one sees stumps of cocoanut trees on the beach as mute evidence of groves that existed before the sea swept them away and a number of the palms overhang the beach itself with part of the roots exposed by the action of the waves. After each severe easterly storm, a



EXCAVATIONS IN PROCESS IN ST. BERNARD MIDDENS, TRINIDAD

a. Clearing ground for trench. *b*. Diluvial deposit covering shell-heaps. *c*. Trench, 10 feet wide, through hummock. *d*. Shell- and ash-layer in deposit. *e*. Ash-layer in deposit underneath layer of *tivela mactroides*. *f*. Sherd *in situ* in shell-layer.

number of the trees fall, owing to the fact that the waves have washed away the soil and sand that supported the roots. The writer has been assured by competent geologists that probably not more than five hundred years ago, the shore ran in a straight line from Mayaro Point to Galeota Point, which would place the St. Bernard middens fully a mile inland.

The St. Bernard deposits cover an area of possibly from six to eight acres of land and lie in what now is a cocoanut grove and what some sixty years ago was a field of sugarcane. Since the abandonment of the village site by the aboriginal inhabitants and the subsequent sugarcane and cocoanut cultivation, the shell-heaps have become covered with a diluvial deposit from one to two feet deep, so that the individual middens can hardly be recognized amid the light undergrowth that covers the soil. The writer was not able to determine the number of the individual hummocks for this reason and had to make his excavations in those places where test holes proved the deposits to be the thickest.

The first process in the excavations was to clear the slope of a hummock of the small brush and to run the lines of a future trench in such a manner that they avoided the cocoanut trees as much as possible. A clearance three feet in diameter had to be given to each tree in order to avoid damaging the roots too much. The accompanying illustration, plate III, *a* gives one an idea of the largest hummock found on the St. Bernard estate which hummock was about ten feet high. The roots of the cocoanut trees interfered to a great extent with the excavations and are no doubt responsible for a considerable amount of breakage of the pottery objects found; also, the writer had to watch the laborers constantly to prevent their chopping off the roots too near to the trees, in which event a number of the palms would have been damaged beyond recovery. It generally was possible to make the trenches from ten to twelve feet wide and to excavate in such a manner that the shells and the soil filled the trench up again back of the laborers, leaving eight feet of the bottom of the trench cleared between the cut and the refilled trench. An excavation of this sort can be seen in plate III, *c*.

The deposits themselves ranged in depth from seven feet to less than a foot and, as has been stated, were covered with from one to two feet of diluvial deposit. Practically no objects were found in the diluvial covering, with the exception of small sherds and shells that had been moved towards the surface at those times when the soil was disturbed in cultivation. This diluvial deposit can be clearly seen in plate III, *b* and the first shell-layer under the diluvium is indicated by the laborer with his cutlass. Under the diluvial deposit came a series of clearly defined shell- and ash-layers showing the various periods of occupation of the village site. These layers reached a large number where the deposits were seven feet thick and, as is shown in plate III, *d*, did not run parallel to the surface of the ground nor did they run parallel the one to the other. It would seem as if the aborigines built their refuse-heaps in a different spot on each successive occupation, although on the same village site, which practice would account for the various angles at which the layers ran. The artifacts were most generally found in the darker ash-layers shown in the illustration. Fairly large fragments of charcoal were met with in the ash-layers and also pieces of pitch or manjak. These latter pieces probably came from the deposits that are found in various places on the east coast of Trinidad. It may be mentioned that the writer excavated a number of pottery fragments that showed signs of having been covered with this pitch; whether these fragments are of vessels in which the pitch was melted or whether the pitch had been put in the vessels to mend them, the writer is not prepared to state definitely, although in one or two instances he is almost positive that vessels had been so restored.

The shells in the shell-layers of the deposits were not mixed with soil and were comparatively clean. The ash-layers, on the other hand, were a sticky gray mass in which bone fragments, shells, charcoal and ashes were mixed. A typical ash-layer is shown in plate III, *e* and underneath this layer can be seen a shell-layer composed of the shells of *Tivela mactroides*. The writer estimated that some 40 per cent. of the shells found in the St. Bernard deposits were of this latter species and some 58 per cent. of the

Donax variabilis, which is the small shell shown in plate III, *f*. In this illustration also is shown a large fragment of a pottery vessel *in situ*. The *Donax variabilis* is locally known as the "chip-chip" shell and is found in enormous numbers on the beach at certain times of the year. The present-day inhabitants gather these and make them into the locally renowned "chip-chip soup," a somewhat sandy delicacy greatly esteemed by the negro population. The aborigines undoubtedly used the chip-chip as one of their staple articles of diet and at those seasons when they were not found in abundance probably used the *Tivela mactroides*, which is found mostly on the sea beach north of Cape Mayaro and, strange to say, is hardly met with on the beach south of the Cape. Incidentally, the writer is of the opinion that the shells of this latter species were "steamed" in order to prepare them for food, in contrast to the chip-chip shells, which were boiled. He bases his contention on the fact that where the chip-chip shells are found clean and white in the deposits, the *Tivela mactroides* shells were always found in a blackened condition. The chip-chip shells are so small that "steaming" them would be out of the question and the only way they can be prepared is to cook them so as to detach the small animal from its shell.

The writer is indebted to Mr. L. P. Gratacap, of the American Museum of Natural History, for the identification of the shells found in the deposits and to Dr. F. A. Lucas, the director of the same institution, for the identification of some of the bones.

The sea shells found in the St. Bernard kitchen-middens are the following:

Donax variabilis Say.—58 per cent.
Tivela mactroides Born.—40 per cent.
Ostrea cristata Born.
Pecten nodosum Linnaeus.
Scarpharca brasiliana Lamarck.
Lucina jamaicensis Lamarck.
Tellina interrupta Wood.
Labiosa canaliculata Say.
Dione veneris Argenville.
Melongena melongena Linnaeus.
Melongena morio Linnaeus.

Strombus gigas Linnaeus.
Cypraea exanthema Linnaeus.
Astralium tuber Linnaeus.

Of fresh-water shells the following two varieties were found:

Ampullaria spixii Orbiguy.
Ampullaria cornu-sarietis Linnaeus.

One species of land-shell was found, that of the

Strophocheilus (*Borus*) *oblongus* Miller.

Besides these shells, a great number of fragments of madrepore coral were found in the deposits.

Of the bones, those that were identified were of the

Deer—*Odocoileus gymnotis*, of the
Collared peccary—*Pecari angulatus angulatus* and of the
Green turtle—*Chelone mydas*.

A number of bones were found of fishes and of some of the smaller mammals which were not identifiable. The St. Bernard deposits also produced a number of crab carapaces and claws and the large crabs found in this neighborhood are even today a favorite food of the inhabitants.

Unlike on the island of Margarita, no split human bones were found in the shell-heaps. The writer found two burials in the middens and the skeletons were found in such a bunched-up position that he came to the conclusion that the dead had been placed in a contracted position, and probably had been buried in their house site. This was not an uncommon practice of the Antilleans and it was usual to afterwards abandon the house and move to another spot.[1] The two burials in question were found in a layer of sea sand under and above which were the usual shell- and ash-layers, evidence that the village site had been used before and after the deaths had taken place. It was not possible to preserve the brittle bones, but, judging from the skull fragments, the heads had been artificially flattened. The writer was especially struck by the appearance of the teeth of both skeletons: they were quite ground down, as in the case of very aged persons, yet did not show a trace of decay.

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[1] Fewkes, J. Walter, "Relations of Aboriginal Culture and Environment in the Lesser Antilles," *Bulletin of the American Geographical Society*, vol. XLVI, no. 9 (September, 1914), page 673.

Some six miles to the south of the St. Bernard estate, on a property called Lagon Doux, also belonging to Mr. Paul Urich, the writer was shown another shell deposit on the southeastern slope of a hill about four hundred feet high and lying a mile inland from the sea beach. These deposits, however, were not thick enough to warrant the conducting of excavations and, due to centuries of rainfall, the middens had been washed down the slope of the hill and distributed over a large area with a resulting breaking-up of the objects into small fragments. A number of shallow deposits were also found by the writer on the extremity of Cape Mayaro, some three hundred feet above sea level. A cursory examination was enough to show that the deposits were too shallow to produce objects that would make excavating worth while.

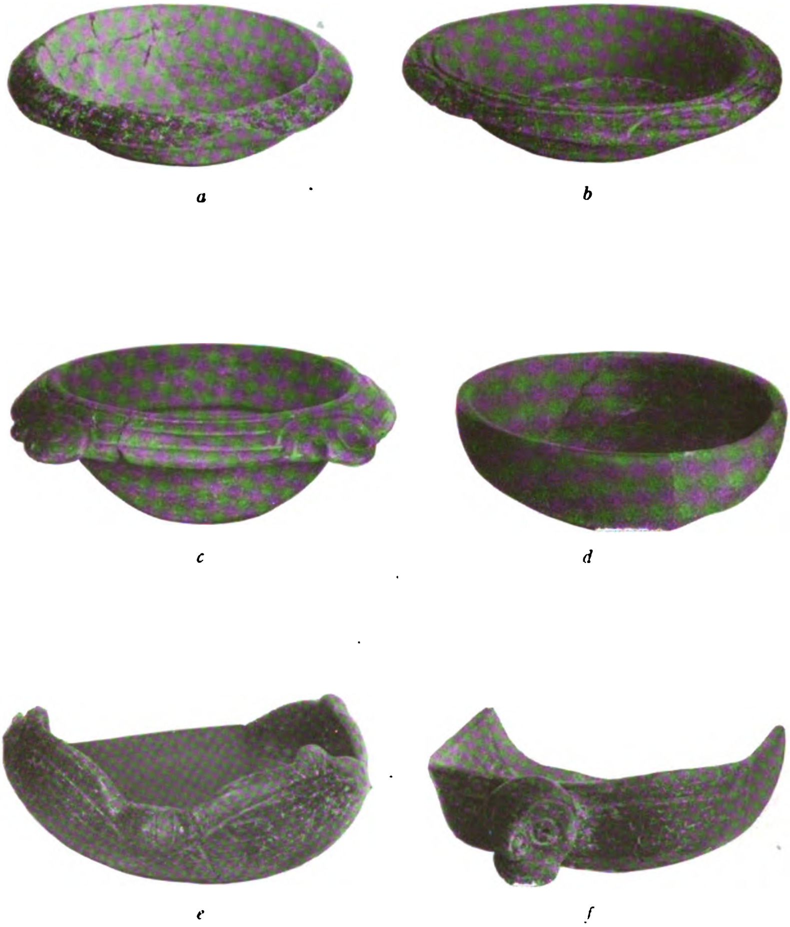
On the Cocal property, about four miles to the north of Cape Mayaro, the writer discovered an isolated shell-heap, twelve feet high with a diameter of about fifty-five feet, composed almost exclusively of the shells of *Tivela mactroides*. This shell-heap presented a curious problem to the investigator, as its height and isolation made it prominent in the landscape. Shallow shell-deposits were found bordering it on all sides but, as in the previously cited cases, the writer did not consider it worth while to excavate in any of these, although test holes and small sherds on the surface of the ground proved conclusively that an Indian occupation had existed here. A ten-foot trench was dug through the isolated mound north and south and another ten-foot trench east and west, forming a cross over the summit of the mound. While a large number of small pottery sherds had been found on the surface of the ground in all directions from the mound, not a single sherd was found in the mound itself. Three hammerstones were found and two small pieces of rock-crystal, the latter evidently having been brought there from the northern mountain range of the island of Trinidad, the only place on the island where these crystals are found according to reputable local geologists. It will be seen, therefore, that this isolated mound, exclusively composed of shells, in which no ashes, or bones and practically no artifacts were found, presents a curious problem compared to the other shell-deposits on the east coast of Trinidad, a problem which the writer is not able to solve.

ARCHAEOLOGICAL RESULTS FROM THE EXCAVATIONS

The most typical of all the various pottery vessels found in the St. Bernard middens is a shallow bowl form with a broad angular rim. It is safe to say that of the sherds found, at least 75 per cent. indicated that they belonged originally to a vessel of this type and this form, therefore, in its many variations, can be taken as being typical of the east coast region of Trinidad.

The simplest form of vessels of this type is the small bowl *a* shown on plate IV. This little dish has a main diameter of 7¼ inches and an inside diameter of 5½ inches and its height is 1⅞ inches. The angular rim itself is ⅞ inch wide and was covered with a red paint by the potter on the upper surface. The body of the bowl is of a buff color and the paint on the rim was probably applied after the firing of the vessel and can easily be washed off. The pottery is ⅜ inch thick. It must be noted that in all vessels of this type the height of the vessel is from ⅓ to ¼ of its outside diameter. The base of this type of vessel is invariably flat but, judging from sherds found, was in some instances extended into a circular stand to give added stability to the vessel. While the specimen illustrated has a painted rim, a large number of bowls of this type were found of which the rim had not been painted and was of the same color as the vessel proper. This then is the simplest form of the type under discussion.

The next addition to the decoration of this type of bowls are the handles and an astonishing variety of forms of the latter were met with in the St. Bernard deposits. In plate V, *a*, can be seen one of these bowls to which has been added two loop handles of a somewhat elaborate pattern. Outside of this addition the bowl does not differ materially from the one previously described, excepting that it is far larger. The diameter of the bowl itself is 12 inches and its depth 4 inches. The thickness of the pottery is ⅜ inch and the ware is buff colored.



POTTERY VESSELS FROM ST. BERNARD MIDDENS

Then again, the potter took the plain bowl of this type and added a design of incised lines to the rim. The simplest form of this were from one to six concentric lines running around the rim. In plate IV, *b*, can be seen a small bowl to which the potter, by way of decoration, has added two incised lines running concentrically around the rim and four extra semi-circular lines from the outside incised lines to the outside of the rim. The main diameter of this small bowl is 7 inches and the inside diameter 5½ inches: the height is 1¾ inches and the ware ⅜ inch thick. This bowl is reddish-brown in color.

In addition to the incised lines, the potter occasionally added nodes or lugs on the rim and an example of this decoration is shown on plate IV, *c*. The rim of this specimen has concentric incised lines and four nodes which are placed equidistant. The nodes probably represent highly conventionalized heads. The specimen under discussion has an outside diameter of 8½ inches, an inside diameter of 5¾ inches and stands 2¾ inches high. The ware is ⅜ inch thick and the vessel is buff-colored.

The most highly decorated form of all is where in addition to the incised lines on the rim, handles of various forms have been added. While only a comparatively small part of the bowl shown on plate V, *b*, was recovered, this specimen may serve to illustrate the highest development of decoration of the type of vessel under discussion. This specimen has an angular rim upon which incised lines have been carved. Furthermore, nodes have been added to the rim and besides these, a handle representing a conventionalized human head. This head is quite massive and it is permissible to state that another head of the same type must originally have opposed it on the rim. The original diameter of this bowl was 12 inches and its estimated height 4½ inches. The ware is ⅜ inch thick and reddish-brown in color.

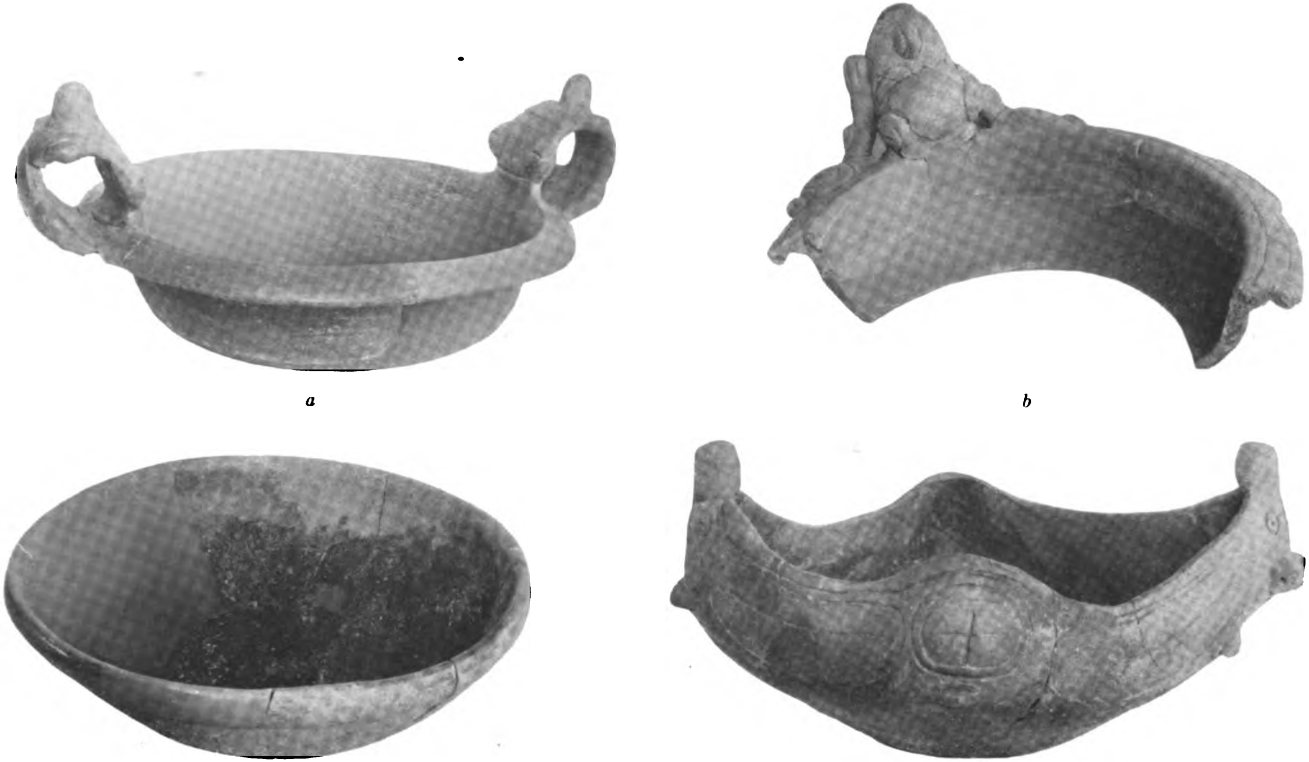
A critical examination of all vessels with angular rims found in the St. Bernard shell-heaps will reveal the fact that each and every one of them can be classified under one of the vessels described above. The vessels range in size from some with an outside diameter of 3 inches and a height of 1 inch to the largest with an outside diameter of 16½ inches and a height of 5 inches.

Yet another type of bowl was found to occur frequently in the deposits and one of these is shown on plate V, *c*. This, of course, is the simplest form of round vessel that it is possible for a potter to make. A number of small differences in outline can be found but fundamentally, the bowls are of course the same. The specimen illustrated has a diameter of 10 inches and a height of 3¼ inches. The ware is ⅜ inch thick and is of a gray-brown color. The rim is slightly thicker than the base part of the vessel. The base of this bowl is indented. The inside of this vessel is partly covered with pitch or manjak, and the writer has already mentioned the occurrence of this latter substance in the shell-heaps. He does not believe that in the case of the illustrated vessel this pitch was employed to mend a break in the bowl, as to his mind, in that event the pitch would have probably been applied to the outer surface of the bowl. The writer presumes that the vessel must have been used as a container for the molten pitch. A number of sherds were discovered, however, that had the pitch applied to the inner and to the outer surface, and cases were met with where the vessel showed signs of having been cracked under the pitch-layers. It is permissible therefore to assume that the aborigines were in the habit of mending their cracked vessels with layers of pitch to strengthen the cracked place and to make the vessel liquid-tight.

A slight variation on this type of shallow bowl is shown on plate IV, *d*, where the convex basal part of the vessel is topped by a perpendicular rim. This rim is 1½ inches high and has been painted red on the in- and on the outside. The body of the bowl is yellow and the entire vessel stands 2⅜ inches high. The diameter of the bowl is 6 inches, and the ware is ¼ inch thick.

This type of vessel, of which the two specimens described above can be considered typical samples, occurred in large quantities in the middens. Occasionally the rim was thickened or painted and the vessels ranged in size from a diameter of 3¾ inches (the smallest specimen of this kind found) to a diameter of 13½ inches.

Perhaps the most unusually shaped vessel found in the St. Bernard deposits is the one illustrated on plate V, *d*. The writer knows of no other vessel like it from the West Indies and considers



POTTERY BOWLS FROM ST. BERNARD

the form unique. The handles of this four-cornered bowl undoubtedly represent the heads of the armadillo, an animal that is frequently found in Trinidad and that is considered a great delicacy by the natives. It may be mentioned that a large number of heads were found in the shell-heaps that represented the armadillo and we judge from this that it was a favorite practice of the aborigines living around Mayaro to thus ornament their pottery vessels. Incised circles ornament the two other terminals of the vessel under discussion and an incised cross can be seen in the one, while the opposite circle contains twelve impressed pits. The bowl is not well made as far as symmetry is concerned. It has a flat base and stands 5 inches high at the rounded terminals. The length of the vessel across the heads is 13¼ inches and it has a short axis of 10¼ inches. The ware is quite thin for such a large vessel, ¼ inch. The color of the bowl is buff. Between the heads and the rounded terminals can be seen panels of incised decorations. There is a small lug on either side of the heads and another lug below the incised circle of the rounded terminals.

Another vessel of elaborate pattern is that shown on plate IV, *e*, a fragment of which is also illustrated *in situ* in plate III, *f*. It is likely that this is one of the so-called turtle-bowls, judging from the shape and design of the incomplete vessel. The head of the turtle can still be seen, but the tail is missing. The four rounded parts of the rim—of which three remain—represent the flippers of the turtle in a conventionalized manner and the outside of these flippers have been decorated with cross-hatched incisions and covered with a dark-red slip of pigment on the in- and on the outside. The rounded parts of the rim are surmounted by a small lug with two incisions. The base of this vessel is flat. The diameter of this bowl is 7 inches and it stands 2 inches high where the head is and 3¼ inches at the highest point of each flipper. The ware is ¼ inch thick and the body of the bowl is painted yellow.

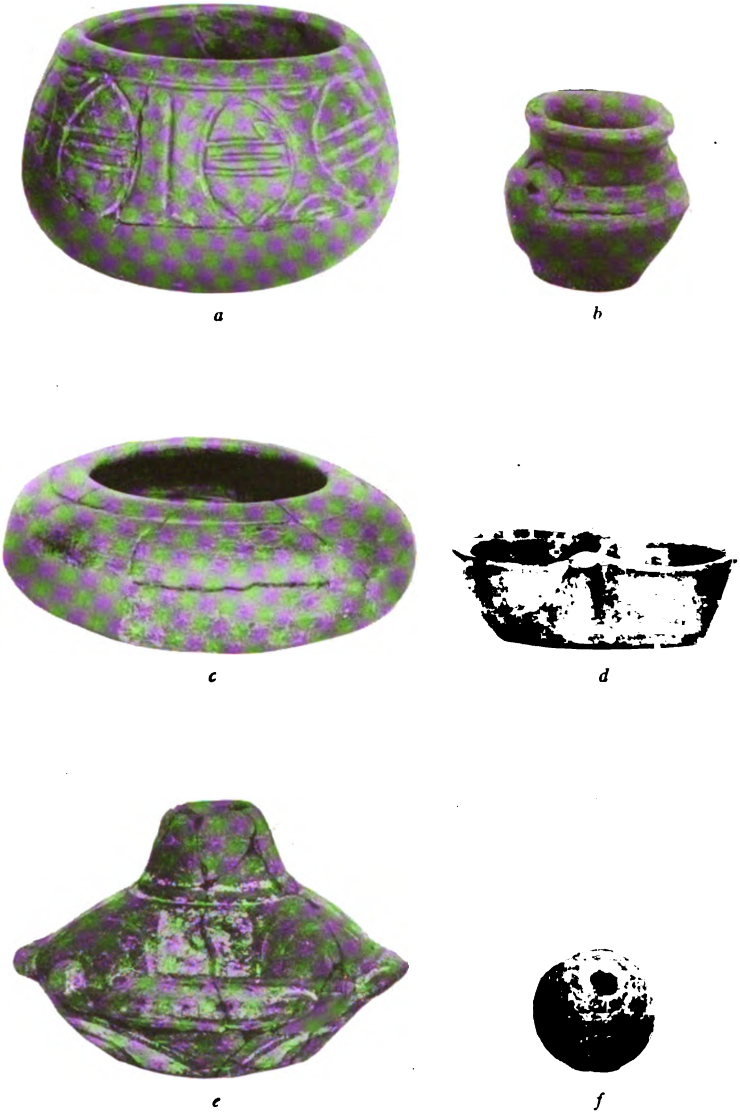
On plate IV can be seen another unusual bowl, *f*, of which but a fragment was found. This vessel originally must have been rectangular in shape and has a flat, round base. A grotesque human head serves as handle at the one end and it would be interesting to know whether the other terminal was provided with a handle and if so, what shape it had. An incised line surrounds the rim on the outside and the rim has been covered with a dark-brown slip of pigment, also on the outside. The grotesque head also has been covered with this dark-brown pigment. The vessel has a width of 6½ inches and an estimated length of about 8 inches. The height is 2½ inches and the ware, brown in color, is ⅜ inch thick.

The small vessel illustrated on plate VI, *a*, is unusual in so far that it reminds us more of the pottery from Santo Domingo and Porto Rico with its incised decoration surrounding the rim in a geometrical pattern. The outline of this vessel was restored from the fragments found and it cannot be determined whether the base was flat or rounded. The largest diameter of this bowl is 4¾ inches and the diameter of the mouth 3¾ inches. The ware is ⅜ inch thick and the bowl is brown in color.

The flat, discoidal-shaped vessel with the comparatively small mouth illustrated on plate VI, *c*, recalls to our mind the sea-urchin and it may well be that the potter derived the contour of this bowl from the shape of this animal. This vessel is unique in shape as far as the West Indies are concerned and no other fragments of like vessels were found in the St. Bernard deposits. The little bowl stands but 2½ inches high and its largest diameter is 6¼ inches, while the diameter at the mouth is 3½ inches. The thickness of the ware, which is brown in color, is ¼ inch. An incised line surrounds the mouth of the vessel and forms the only decoration.

The smallest vessel found is the one illustrated on plate VI, *b*, which stands but 2 inches high. It can hardly be supposed that this miniature vessel was intended for a container. The vessel is provided with two small, perforated lugs. An incised line surrounds the rim and two more incised lines the body of the vessel. The ware is brown in color and ¼ inch thick. No sherds were found of other vessels of this small size.

A number of double paint-bowl vessels were found in varying sizes and the smallest of these is illustrated in plate VI, *d*. The length of this specimen is 4⅜ inches and the height 1½ inches. The ware is brown in color and $\frac{3}{16}$ inch thick. There is a small lug on each side of the wall separating the two bowls.



POTTERY OBJECTS FROM ST. BERNARD REFUSE HEAPS

The small perforated globular object with the incised lines, shown on plate VI, *f*, is either a pottery bead or else was used as a spindle whorl. This is the only specimen of this kind recovered from the Mayaro kitchen middens.

A very large number of pottery heads and handles were excavated in the shell-heaps. These do not differ materially from those illustrated in Dr. Fewkes' paper on the archaeology of Trinidad[1] or from those figured in the writer's paper on the archaeology of Margarita Island.[2] The heads shown in Dr. Fewkes' paper, from Erin Bay on the south coast of Trinidad are of a slightly coarser manufacture, but resemble the Mayaro specimens in type. It may be noted in this connection that all the St. Bernard pottery was of a more delicate finish than that from Erin Bay and the vessels were not as thick as the vessels collected by Dr. Fewkes. The writer only figures two of the heads collected in the St. Bernard deposits, as these two heads are uncommon in type and are of a variety not previously figured. Specimen *a*, plate VII, is an extremely massive head and larger than the usual types and represents a conventionalized human head. The mouth and the eyes are depicted by incised lines: small impressed pits serve to indicate the nostrils and the ears also have pits to indicate perforations. This specimen stands 4 inches high and is dark-gray in color. Specimen *b* of plate VII is a flat handle and also represents a conventionalized human head. No other specimen like it was found or is known to the writer, from the West Indies. The head is flanked by large scroll-like ornaments. The forehead is ornamented with cross-hatched incisions, as is the back of this handle. The height of this specimen is 3¾ inches and it is dark-brown in color.

An extremely large number of spool-shaped pottery rests were found, of varying sizes and two of these objects, *c* and *d*, are illustrated on plate VII. *c* has an inside diameter of 2⅝ inches and an outside diameter of 4¼ inches and stands 1½ inches high. The ware is ⅜ inch thick and is brown in color, covered with a red slip of pigment. *d* has an inside diameter of 2½ inches, an outside diameter of 4⅝ inches and stands 2½ inches high. The ware is ½ inch thick and is brown in color covered with a red slip. Incised lines decorate the body of this rest. On *c* can be seen single and double nodes which are placed symmetrically around the rim of the object. Some of the pottery rests found were ring- instead of spool-shaped and an astonishing variety of ornamentation was found in the objects of this class.

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[1] Fewkes, J. Walter, *op. cit.*, plates XVI, XVII, XVIII, and XIX.

[2] Booy, T. de, *op. cit.*, plate II.

The writer has purposely left a discussion of the highly decorated painted pottery to the last. It has already been stated that many of the vessels and sherds found showed that a superficial slip or wash of paint had been applied to the ware. This, however, had either been applied after the firing of the clay or else had been applied before the firing and had not become a very substantial part of the ware. A very large number of fragments were found, however, upon which was found a heavy slip of solidly-applied pigment in geometrical designs and these fragments seem to have been subjected to great heat in the firing and are far less brittle than the specimens previously described. Three of these sherds were found on Margarita Island[1] and these sherds were identical with some eight hundred fragments found in the St. Bernard middens. It must be noted that on no other part of Trinidad have sherds of this class been found to the writer's knowledge. What makes the finding of this kind of pottery all the more interesting to the archaeologist is the fact that it was not possible to reconstruct a single vessel from the sherds found: not only were no sherds of one same vessel found but no two sherds were found even that fitted on to each other. In the object illustrated in fig. 62, the reader will note the presence of "breaks" in the object, but these breaks

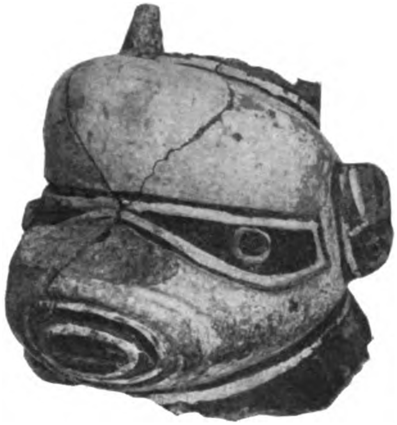

FIG. 62.—Pottery monkey head.

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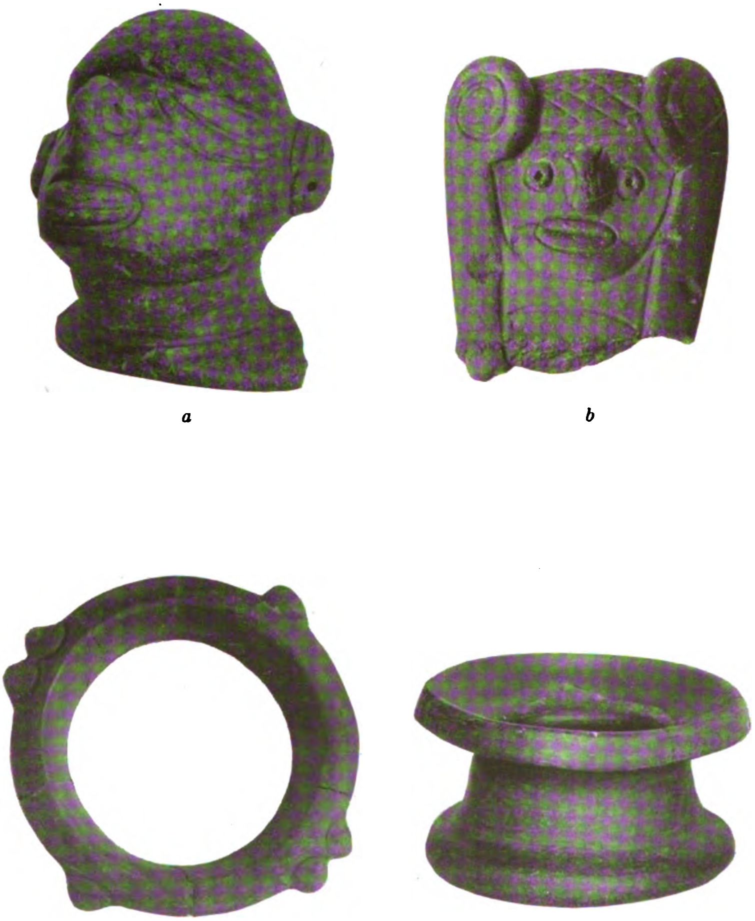
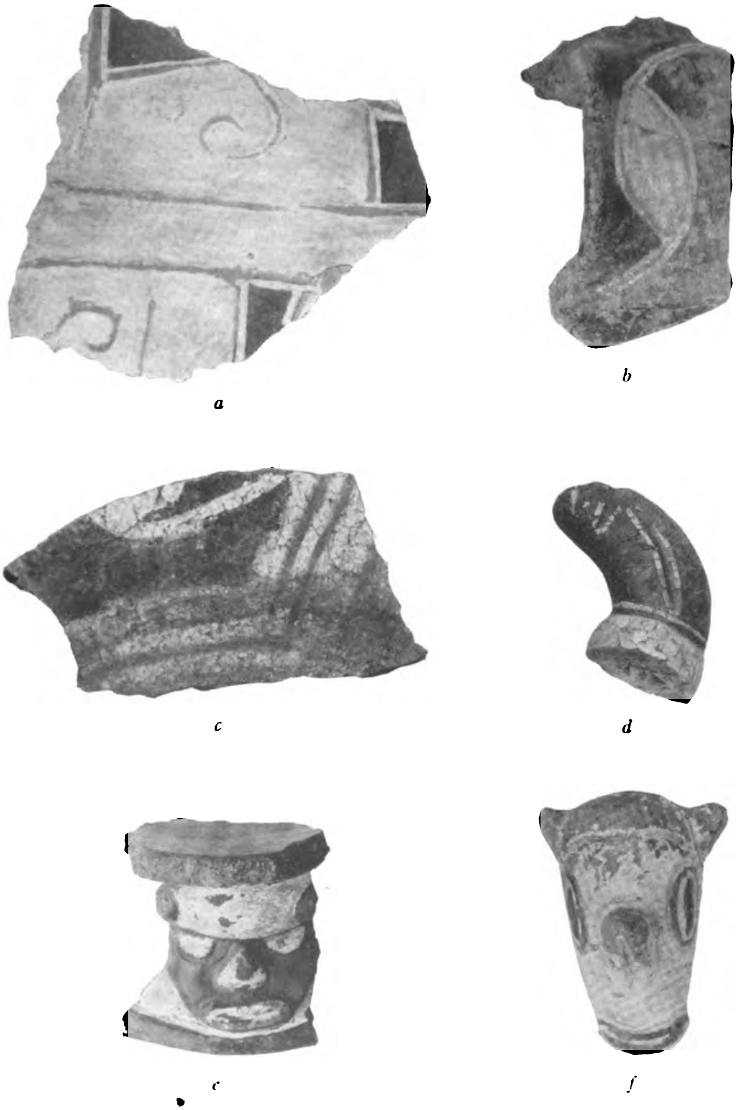
[1] Booy, T. de, *op. cit.*, page 20, fig. 7.



a *b*

c *d*

POTTERY HEADS AND POTTERY RESTS FROM ST. BERNARD MIDDENS



POTSHERDS, LUGS, AND POTTERY HEADS FROM ST. BERNARD MIDDENS

were the result of the excavation and not of the later matching together of the pieces. It would seem, therefore, as if the aborigines that made this particular class of pottery not only broke the vessels up, but took pains to distribute the resulting fragments over a large area. The writer can offer no explanation for this seemingly meaningless custom.

On plate VIII, *a* and *c* can be seen two of the sherds under discussion and a superficial examination will convince the reader that these sherds belong to a totally different class of pottery than the kind previously discussed. The ware appears to be far better baked and is not so fragile; the slip is thick and consists of red, brown, yellow, black and white pigment. The designs are applied in geometrical patterns and it is to be regretted that not enough of one design is left on any one sherd to enable the writer to work it out. The vessels were frequently provided with handles, upon which the potter also placed a painted design. One of these handles is illustrated on plate VIII, *b*. Nodes or lugs also were added to the vessels; one of these is shown on plate VIII, *d*. Then again some the vessels must have had pottery-heads, of which a grotesque sample is shown in *f* of the same plate. Another head, *e*, is also shown, which head was part of the base of a vessel.

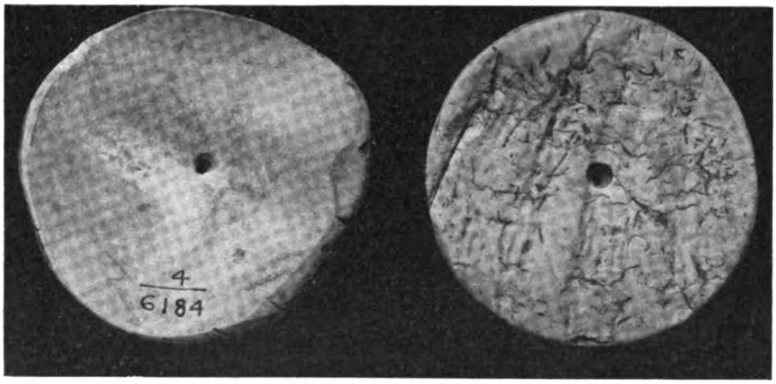


FIG. 63.—Large shell-disks.

Judging from a number of the sherds found, some of the vessels served as containers for liquids and had a highly ornamented head on top of the vessel proper which served as a spout. The best specimen found is shown in fig. 62 and probably represents a monkey head. Only two colors, white and red, were used to decorate the gray ware of this object, which stands 4 inches high.

The vessel shown on plate VI, *e*, does not belong by rights to the class of pottery described above, although it has received the same painted slip. The ware of this object however is brittle and not well baked and resembles the ware of the vessels first described in this paper. The decoration is red and white in geometrical design over a brown ware and the writer is inclined to believe that the potter intended this vessel to represent a turtle. Four nodes represent the flippers and the tail is shown. The head has been broken off. This vessel stands 4 inches high and has a main diameter of 5½ inches. The diameter of the mouth is 1¼ inches.

A large number of fragments of pottery griddles for the baking of cassava bread were found in the shell-heaps, which fragments did not differ from objects of the same class found throughout the West Indies.

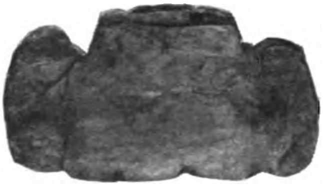


FIG. 64.—Pendant of Amazon stone.

Two large shell-disks were found, shown in fig. 63, which undoubtedly were objects of personal adornment. These disks or gorgets have a diameter of 2¼ inches and are about 3/16 inch thick.

Yet another pendant ornament is shown in fig. 64. This little object is made of Amazon stone, is 1⅝ inch long and 1 inch wide and ⅛ inch thick.

MUSEUM OF THE AMERICAN INDIAN, HEYE FOUNDATION,
NEW YORK CITY

A PRE-LENAPE CULTURE IN NEW JERSEY

By E. W. HAWKES and RALPH LINTON

THE existence in New Jersey of archaeological remains older than those of the historic Lenape has been recognized for a long time, but our knowledge of the age, geographical distribution, and content of the more ancient culture, or cultures, is still very slight. In the hope of throwing further light on these problems, the Museum of the University of Pennsylvania, with whose work the authors have been connected, has carried on investigations in the central and southern parts of the state during the past two summers, and excavations have been made at several sites, which have yielded important results. A description of the first of these, near Masonville, has already been published as no. 3, vol. VI of the Museum's Anthropological Publications. A site near Medford, excavated during the past summer, has revealed what is apparently an extension of the culture found at Masonville, and the two present enough points of similarity to justify their description together.

At both the Masonville and Medford sites, which are about eight miles apart, the geological strata appear to be identical in nature and arrangement. Their order is as follows:

1. A surface layer of black soil, six inches to a foot in depth.
2. A layer of yellow sand three to six feet in depth.
3. A layer of white sand about a foot in depth.
4. A layer of cretaceous marl extending to below the ground water level.

The consulting geologist, Dr. A. P. Brown of the University of Pennsylvania, declared the surface black soil to be of recent origin, the yellow sand of glacial or immediately post-glacial age, and the white sand of glacial age.

Artifacts of the recent Lenape type were obtained at both sites from the black surface soil, but were not found at any of the lower levels. At Medford, many artifacts differing in type and material from those of the recent period were found scattered through the yellow soil from top to bottom, while at Masonville the only objects found in the yellow sand were about a dozen points and some sherds of a peculiar pottery, all of which occurred at a single level about midway between the surface soil and the white sand. No traces of fires or other signs of occupation were found in the yellow soil at either site, but at both sites, *a distinct and well-marked level of occupation occurred at the juncture of the white and yellow strata.* This was most strikingly shown by the presence of fire pits, the ashes and charcoal of which had discolored the underlying white sand, and in some cases even the marl, while no similar discoloration extended up into the overlying yellow soil. Additional proof was offered by the fact that by far the greater part of the artifacts found occurred at this level. At the Masonville site there was a great central fire pit, around which caches of objects had been systematically arranged. These caches were of two types: the more common consisting of one or two bannerstones, associated with concretions of peculiar form, fossils, etc.; while deposits of less frequent occurrence contained two large argillite spear heads or blades associated with a handful of bone fragments. Certain of these bone fragments were identified by a paleontologist as belonging to the bear, deer, and wild turkey. All these caches were found at the juncture of the yellow and white strata At the Medford site many small fire pits (fig. 65) were found around which were scattered artifacts of all sorts, finished and unfinished, including three bannerstones. While the great bulk of the artifacts at this site, as in that at Masonville, occurred at the juncture of the white and yellow sand, the objects found in this position were indistinguishable from those found scattered in the overlying yellow soil. The objects from both sites were apparently identical in type, material, and

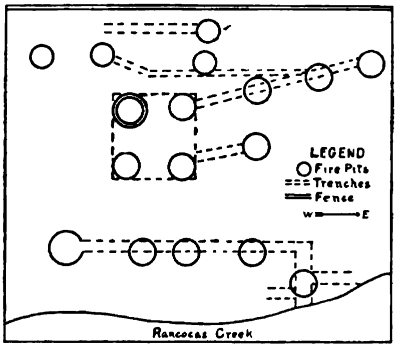
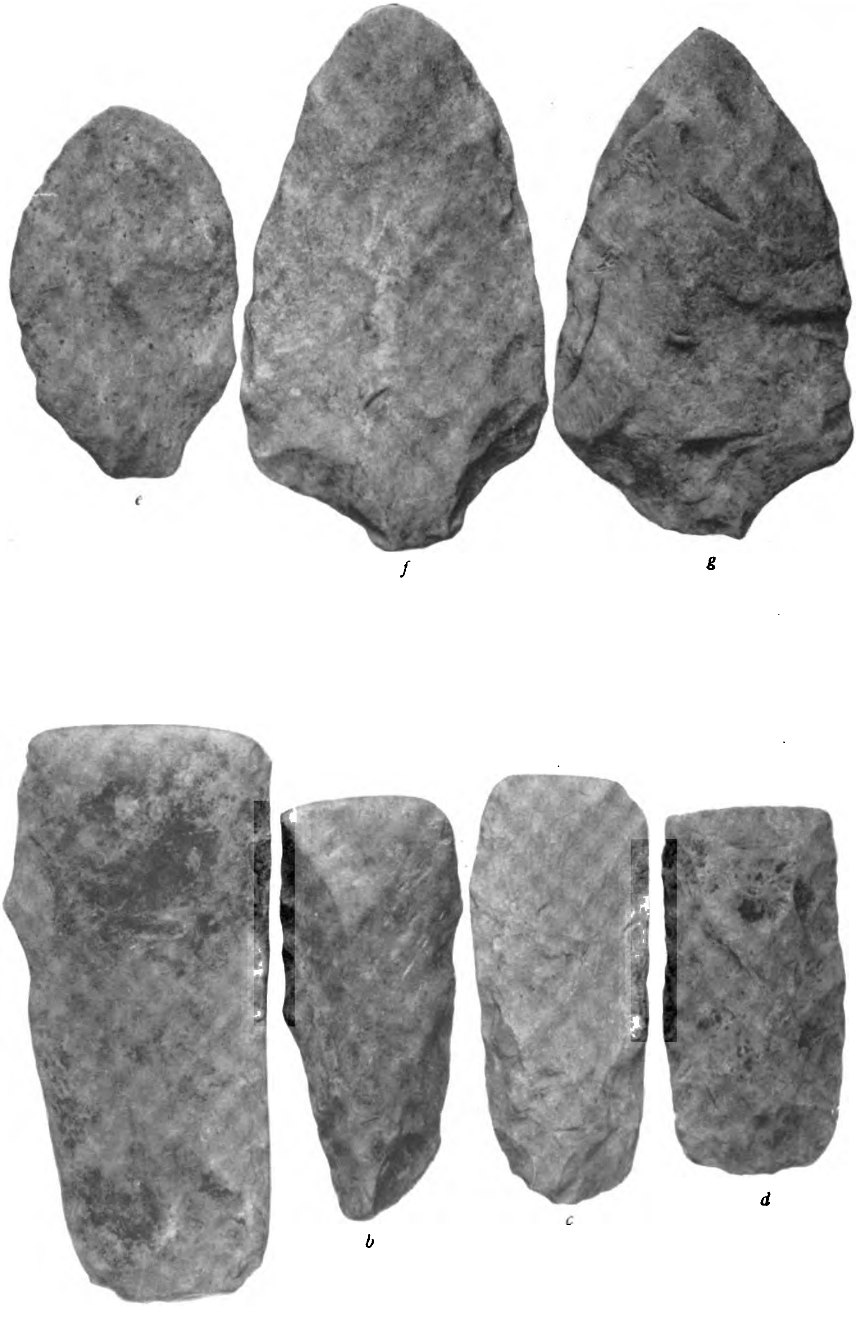


FIG. 65.—Diagram of excavation at Medford, N. J.



CHIPPED CELTS AND BLADES

workmanship. Sherds of pottery of the sort found in the yellow soil at Masonville were found on the surface of the white sand at Medford. There were no signs of disturbance in the yellow soil at either site; and the conclusion seems inevitable that *the artifacts found on the white sand were deposited, and the fires built, at some time prior to the deposition of the yellow soil.*

Excavations made near Indian Mills revealed artifacts similar in type and material to those found at the above described sites scattered through a stratum of yellow sand underlying the black soil of the present surface, but did not show any recognizable level of occupation. Excavation of shell heaps near Tuckerton also disclosed artifacts of this sort in shell deposits underlying those of the recent Indians and separated from them by thick layers of undisturbed black soil. No excavations have so far been made in the extreme southern part of the state, but there is an abundance of material in local collections which is quite indistinguishable from that excavated by the authors. Not all the artifact types found at Masonville and Medford occurred at Indian Mills and Tuckerton, and the excavations at the latter sites were not of sufficient extent to give final proof of either cultural continuity or difference, but all the types found there were, however, present at the two more important sites as well.

At all the sites excavated the artifacts of the recent Indians, and those of the ancient inhabitants, occurred in sharply differentiated strata, the former being associated with the recent surface soil, the latter with the yellow and white sand layers. Enough material has been collected from the ancient levels of occupation at Masonville and Medford to render possible a tentative description of the implement types of the older culture, as follows:

I. *Chipped Celts* (plate IX, *a*, *b*, *c*, *d*). These implements, all of which have been found at the surface of the white sand, represent a hitherto undescribed type for New Jersey. They are of argillite, or some closely related mineral, chipped, and with the cutting edge ground. In one unfinished specimen the edge is unground. A slight projection has been left on the side of another specimen, apparently to facilitate hafting. Still another is of adze form. Length, three to eight inches.

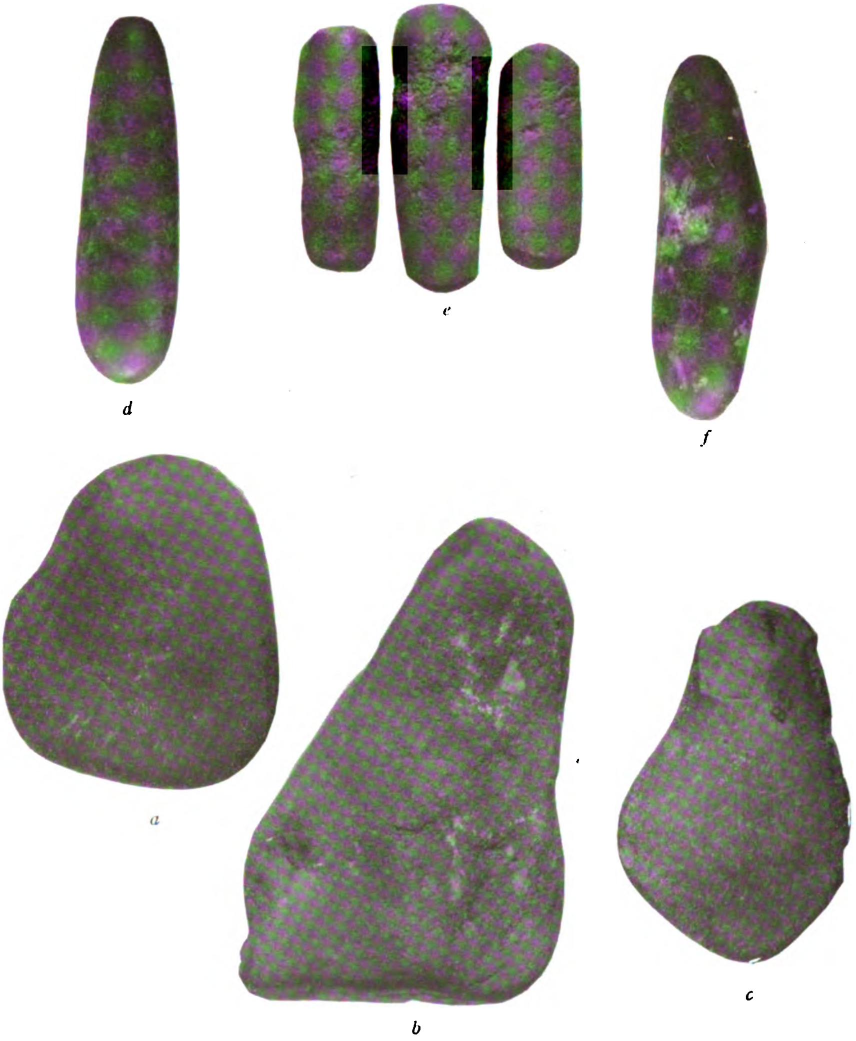
II. *Hoes* (plate X). This name has been tentatively applied to certain crude implements found on the surface of the white sand at Masonville and Medford, and in the yellow soil at Indian Mills. They are of crude workmanship, roughly oblong in shape and concave on one side. Some of the specimens are quite thin and fragile at one end, and do not seem adapted for heavy work. They may have been used for digging in the sand. None of them show signs of hafting.

III. *Hand Hammers* (plate XI, *a*, *b*, *c*). This name has been applied to a series of pear-shaped glacial pebbles found in the caches at Masonville. In every case they were associated with problematical objects or bone fragments. All showed signs of battering along the lower edge, but were otherwise unworked. Apparently they were not hafted.

IV. *Small Cylindrical Stones* (plate XI, *d*, *e*, *f*). Several of these peculiar objects were found on the white sand at both sites, those at Masonville being in the caches. A few were of sandstone, but the majority were of harder, tougher material. All appeared to be natural forms, but in every case they showed signs of battering at one or both ends, while some specimens had been highly polished. One sandstone specimen had had a shallow groove pecked about its middle. Another, found at Masonville, showed traces of red paint. Their average length was about three inches.

V. *Large Blades* (plate IX, *e*, *f*, *g*). These are large leaf-shaped blades which occurred in the caches at Masonville, both on the white sand and in the yellow soil at Medford, and in the yellow soil at Indian Mills. They are of crude workmanship, but quite symmetrical in outline. This would seem to be the commonest of the large implement forms.

VI. *Knives* (plate XII). Under this general head have been grouped two distinct types of implement. 1 (*a*, *b*, *c*). This is a very crude form made by striking off a few large chips from one side only of a larger flake. The base is so thick as to make hafting improbable. 2 (*d*, *e*, *f*). This form is thin and carefully worked, with a distinct stem used for hafting. The most striking feature of these implements is their lack of bilateral symmetry, one edge



HAMMERS

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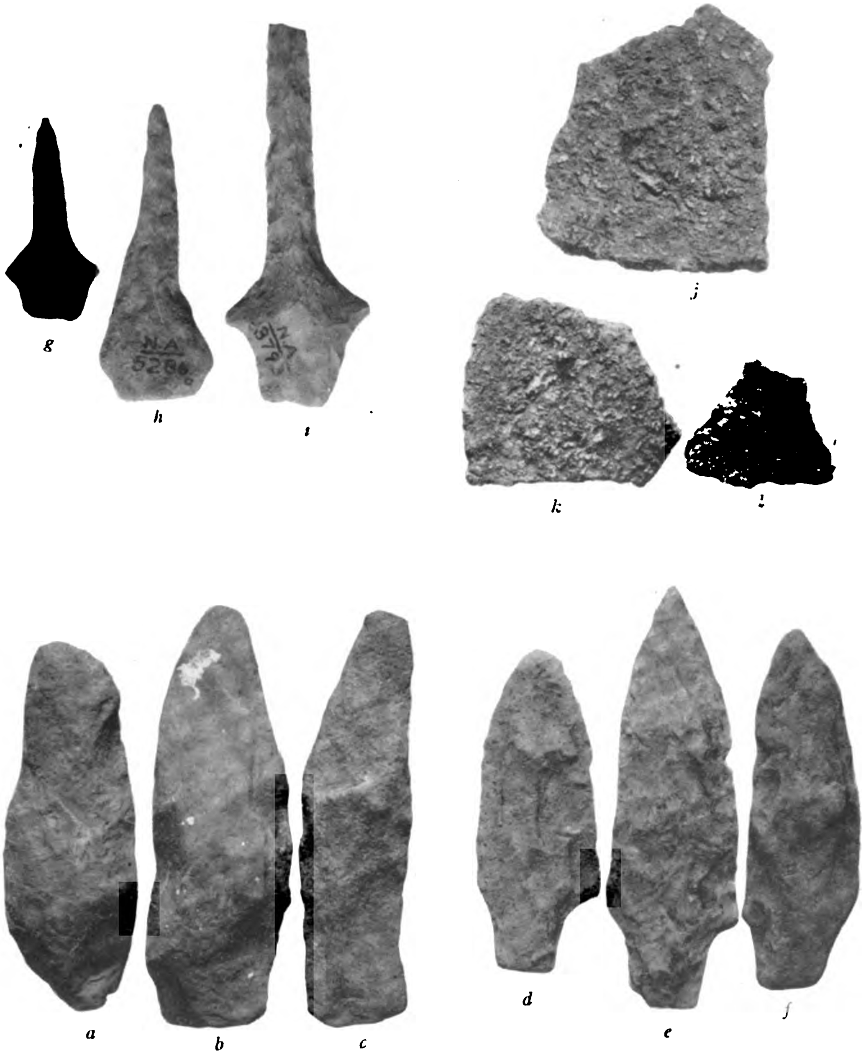


KNIVES, DRILLS, AND POTSHERDS

being considerably more curved than the other. Both forms were found on the white sand at Masonville and Medford, and the first in the yellow stratum at Medford. They vary in length from three to five inches.

VII. *Ceremonial Points* (plate XIV, *a*, *b*). Under this head have been grouped three large spear heads, found in the caches at Masonville, which are so far superior in workmanship to the ordinary implements of the pre-Lenape inhabitants that they belong in a class by themselves. Two of the specimens are of argillite, the third of a bluish quartzite. They are characterized by large size, perfect barbs and stems, and general beauty of outline. The blades are extremely thin, with evenly chipped edges. In view of the rather refractory material used, their workmanship seems to compare favorably with that of any implements found on the recent village sites of the region.

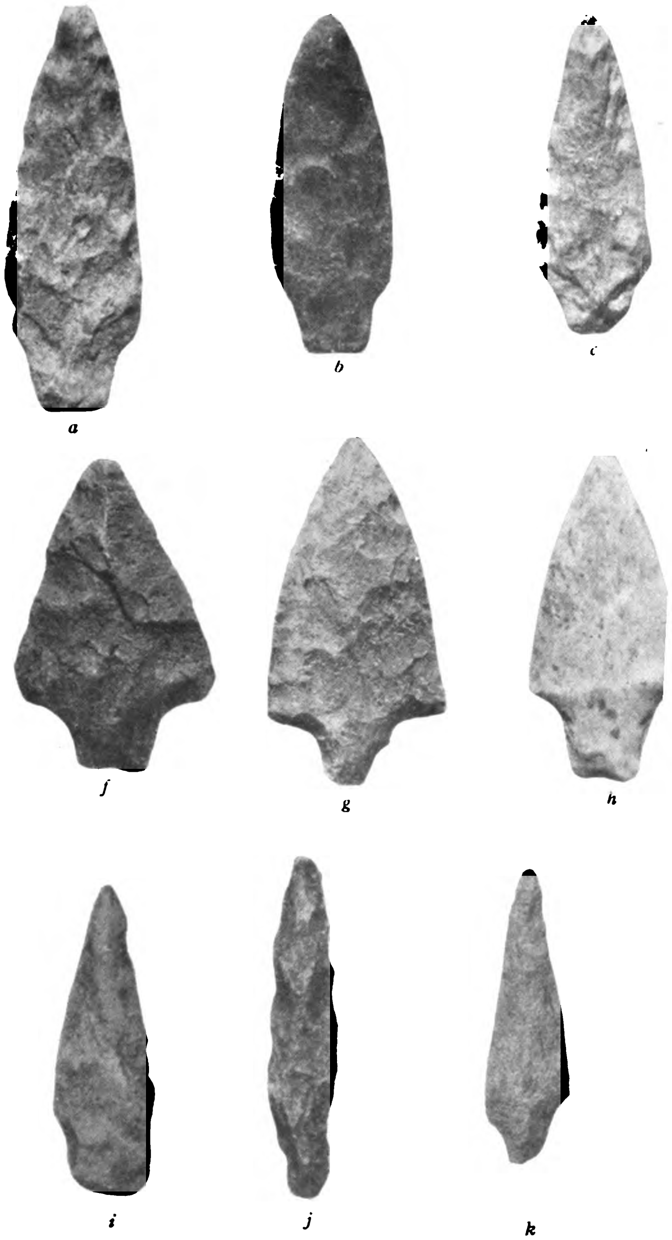
VIII. *Small Projectile Points* (plate XIII). Large numbers of these artifacts, which might have been used as arrow or javelin heads, were found on the surface of the white sand at Masonville and Medford, and in the yellow soil at both places and Indian Mills. Several were also obtained from the lower shell layer at Tuckerton. On the basis of form, these points fall into three main types. 1 (*a*, *b*, *c*, *d*, *e*). The great majority of the smaller points are of this type, which is characterized by a thick midriff, and the absence of well-marked barbs and stems. In outline they are almost diamond-shaped. In length they vary from less than an inch to four inches. 2 (*f*, *g*, *h*). This type, which is roughly triangular in outline, with well-marked barbs and a short stem, is not uncommon in the later Lenape specimens. The points of this form found were much more uniform in size than those of the preceding type, the average length being about two inches. 3 (*i*, *j*, *k*, *l*). Thick, roughly made points, with very heavy midriff and narrow blade. They have very small barbs, or are quite barbless. This is the type called "fish spears" by Mr. Abbott and Mr. Volk.

IX. *Drills* (plate XII, *g*, *h*, *i*). A number of the long narrow points with broad bases, to which this name is popularly applied, were found on the white sand and in the yellow soil at Medford.

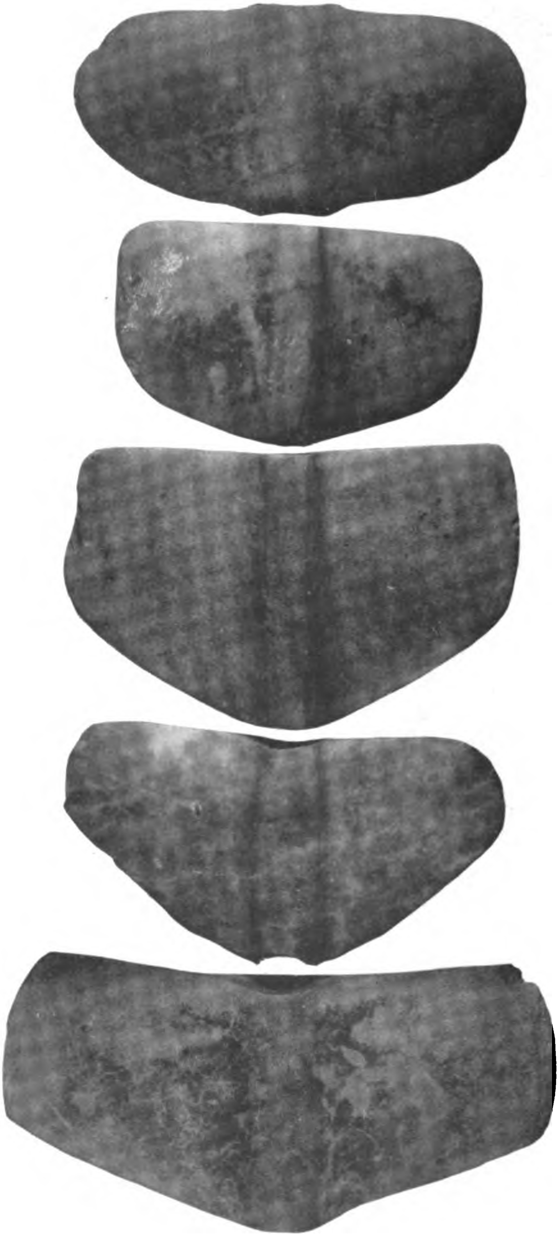
The artifacts obtained from the yellow soil at Masonville also included a drill, (*i*) although none were found at the lower levels.

X. *Bannerstones* (plate XIV, *c*). Mention has already been made of the great number of bannerstones found on the white sand at the Masonville site, and of three others, including one unfinished specimen, obtained from the lowest level at Medford. The presence of these objects in the lowest stratum presents an interesting anomaly, for in size, beauty, and perfection of workmanship they surpass anything of their sort hitherto reported from the state. No other artifacts made by the processes of pecking, grinding, and polishing were found at the lowest level or in the yellow soil of either site, but a comparison of the bannerstones with the chipped implements would seem to indicate that the ancient workmen were, if anything, more skillful in the less used technique. The authors were at one time inclined to believe that the bannerstones might be importations from some neighboring area of higher culture, but the discovery of an unfinished specimen would militate against this. Moreover, the forms present agree more closely with those found on recent village sites in the northeastern United States than with those from any other region. The entire series of bannerstones show a marked uniformity in shape and workmanship, only two specimens showing decided differences from the type. The average length of those found at Masonville was two and four tenths inches, with a maximum wing width of six inches, and a minimum of two and eight tenths inches. The longitudinal perforation had been made with a reed and sand, the drilling having been done in some cases from one, in others from both, ends. The internal diameter of the boring was remarkably uniform in all the specimens, nineteen showing a variation of less than four one-hundredths of an inch. A number of minerals were used, not all of which have been determined.

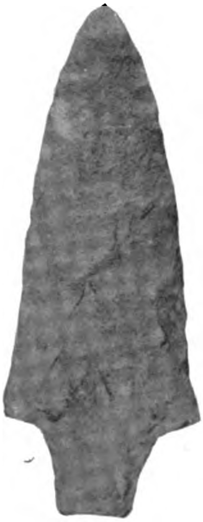
Like the recent Lenape, the ancient inhabitants seem to have been pottery users (plate XII, *j*, *k*, *l*). The ancient ware is, however, of a distinctive character. It is made from a mixture of clay and pounded steatite, the latter composing more than half the bulk. The cementing clay is bright red in color, and the fragments



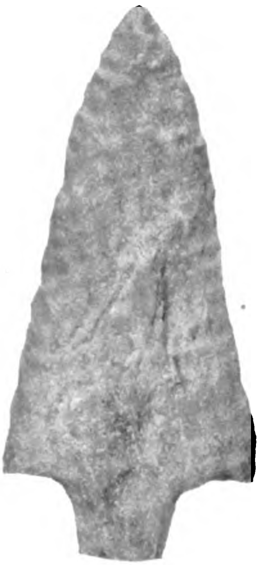
PROJECTILE POINTS



c



b



a

CEREMONIAL POINTS AND BANNERSTONES

found were so soft and friable that it seems improbable that the ware was ever subjected to any high degree of heat. No whole vessels were found, but the shape of the fragments would seem to indicate that they formed part of round or oval pots, with straight sides and flat bottoms. Pieces of this pottery were found at the lowest level at Medford, and in the yellow soil at Masonville. Other fragments, said to have been found near Camden, were preserved in a private collection inspected by the authors.

It can be seen from the foregoing descriptions that the ancient culture was of a fairly high order. Pottery was used, and the artifact types show considerable differentiation. It is differentiated from that of the Lenape by the presence of certain forms and the absence of others. Thus chipped celts and adzes, if they occur at all on recent village sites have never been described. On the other hand, no pestles, mortars, mullers, grooved axes, or indeed any implements made by the pecking and polishing process (other than bannerstones) have so far been found at the lower level. As can be seen from the detailed description given above, the older pottery also contrasts sharply with the well-baked, sand-tempered ware of the Lenape. Still another contrast is offered by the materials employed by the two peoples in the manufacture of their chipped implements. The Lenape used a wide variety of minerals, with a preference for jasper, quartzite, flint, and quartz. In some sections argillite was extensively used, but apparently always in connection with some of the minerals above mentioned. The older inhabitants seem to have employed argillite, or other materials so closely similar as to be indistinguishable to the layman, to the practical exclusion of all other substances. This material does not seem especially well adapted to implement making, for it is coarse grained and chips unevenly, and its use seems explicable only on the basis of a lack of other workable stone. Much of the crudity displayed by the ancient artifacts is no doubt directly referable to the poorness of the medium. Argillite, when exposed to the action of the ground water, undergoes a marked chemical change, and many of the specimens found had turned on the surface to a clayey substance soft enough to be easily scratched with the finger nail. This patination gives an appearance of great age, although nothing is actually known about the rate at which the transformation takes place.

In view of the cultural differences which we have here pointed out, it seems certain that there have existed in New Jersey at least two cultures; while the presence of both at the same sites, but in different strata, would prove that here at least they were not contemporaneous. The surface remains may with some certainty be referred to the historic Lenape, while those from the lower level can be ascribed to some people who were their predecessors. Whether the finds of implements, similar in type and material to those found by us at the lower level, which have been reported from various parts of the state, mark geographical extensions of the culture present at Masonville and Medford we do not feel in a position to say. The period at which the ancient culture flourished is also uncertain, ***but there can be no doubt that it existed, in fully developed form, at a time prior to the deposition of a thick stratum of yellow sand, which is supposedly of glacial or immediately post-glacial origin.*** Needless to say, the rich content of the ancient culture is at variance with all preconceived ideas of the status of man in America at that period.

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SOME CONCLUSIONS BASED ON STUDIES IN CEREBRAL ANTHROPOLOGY[1]

By C. W. M. POYNTER

DURING the past five years the Department of Anatomy, University of Nebraska, has been particularly interested in cerebral morphology. We have had opportunity to study about one hundred and fifty brains collected from the material which has come through the usual channels to the department. From this material we have been able to collect a number of brains of Negroes with marked negroid characteristics. These have formed the basis of our racial comparison with the Caucasians until recently, when we secured several brains of Mexicans and one Indian. Doctor Keegan, who is interested with me in this work, has reported his study of the Indian brain and has just completed, through the kindness of Dr. Hrdlička, the study of three other Indian brains.

A review of the literature of cerebral anthropology would give one the impression that the field is an unprofitable one. Relatively little work of a truly anthropological character has been done, and in the light of our limited knowledge of morphology it is premature to conclude that the brain presents no ethnic characteristics. At first one is inclined to ask why so much energy has been expended on craniology and the brain been so neglected. This is explained, however, when we consider that racial studies are of a very special type and must be based on a thorough understanding of morphology. Our knowledge of the skull is relatively ancient; Galen gave a very satisfactory description and the work of Vesalius left little for later observers to add, while in the case of the brain, the first fact of cerebral localization was pointed out by Broca less than fifty years ago. Because of the lack of understanding of the anatomical structure, brain studies have been largely empirical records of observable variations and elaborations of extravagant theories based on uncertain evidence. It is not surprising that so thorough a student as Doctor Mall should conclude that,

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[1] Read before the American Anthropological Association, New York, 1916.

> With our present crude methods, the statement that the negro brain approaches the foetal or simian brain more closely than does the white is entirely unwarranted.

The question of brain weight which received so much attention at one time, we know now may be dismissed with the statement that the weight of the brain, independent of other data, is without significance.

Cerebral study is very difficult because of the number and complexity of the functions which are expressed in its structure. The evidence at hand seems to suggest that somatic, psychic, and racial characters are expressed in the pattern of the sulci and gyri and the problem in both morphology and anthropology is to discover a method of recognizing and separating these influences. These difficulties are, however, no more insurmountable than some of those which have already been solved in neurological work and should, it seems to me, command our interest and attention.

Variations in fissures and convolutions are so numerous in kind and degree that it has proven impossible statistically to establish a type of variation for race, sex, or degree of mental attainment. Perhaps it would be better to say that we do not know the significance of variations which fall outside of the fissural pattern which we recognize as common to all brains. Does a certain type of mental development tend to produce a certain fissural pattern, or is a child with a certain fissural pattern capable of receiving training more easily along some special line than another without this pattern? If a race is found to possess certain particular fissural characters are they due to psychic or environmental influences, or are they definite morphological characters which have evolved with the evolution of the race? These and like questions cannot as yet be answered positively, but I feel that we are nearer the answer than we were a few years ago.

In an organ so extremely specialized as the brain there is undoubtedly morphological expression for both the physical and psychical functions, but to appreciate these we must use methods of examination very much less crude than those formerly employed in brain examination. Also the method which has been most frequently used in the study of races, that is the selection of a number of arbitrary characters, and the tabulation of their percentage of variation, will not yield profitable results because these characters have no significance. We must not expect to find in the brain racial characters as obvious as prognathism or pigmentation.

Another problem which adds to the difficulties in this field is that of development. The suggestion has frequently been made that the brains of the so-called lower races present a greater variety of variations which suggest the embryonal or simian condition. This would be of special value in racial studies if it could be proven true, but unfortunately we know very little of the developmental processes of the brain cortex and those characters which we are accustomed to speak of as embryonal are determined from a superficial study.

It seems to me that, notwithstanding the difficulties which I have just reviewed, the field of cerebral anthropology offers an interesting and profitable field for research. Most of the work of the past, with the exception of the records it has furnished which may possibly be valuable for later comparisons, has its greatest value in showing us the futility of research in certain directions. In the recent work of Elliot Smith however we have a suggestion of how the newer facts concerning the brain may be made use of in the interpretation of fissural variations which heretofore have been meaningless.

This work of Smith is valuable because it deals with a known functional area, and while it is based on a small amount of absolute evidence of the primary relation between sulci and cortical functional areas, the results of its application are so convincing that one is bound to accept the principle. Briefly stated, his work consisted in aking the area striata, which is the most definitely circumscribed area of the cortex, and by comparison of this area in man and apes determine the way in which brain development is expressed on the brain surface. In the light of this work the direction of a fissure takes on a real significance.

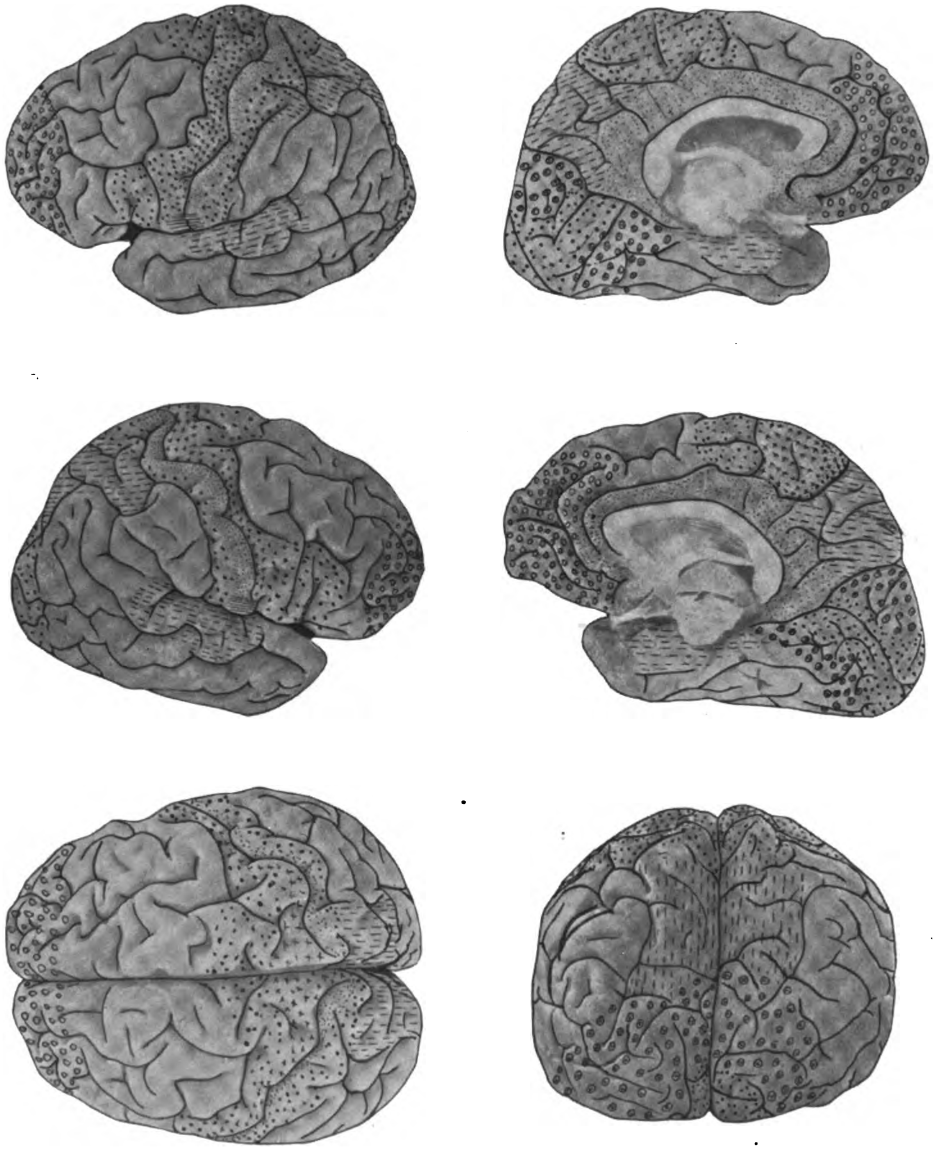
The method is not capable of immediate application to all portions of the brain both because of our lack of knowledge of morphological and functional areas of many portions of the cortex and because other areas are not definitely defined macroscopically as is the area striata. In addition to this the elements affecting development in different portions of the brain are so complex that perhaps they do not always react against each other in the same way, so that in some parts the morphological picture will always remain incomprehensible, but if this be true it will be in the smaller details and still leave much for us to expect from the study.

The experimental work on the cortex which has recently occupied the attention of several laboratories throws light on our problem not only in morphology but on the somatic functional side as well. Doctors Sherrington and Mott, by experimental processes in localization on the ape's brain, have reached the conclusion that,

> Habits and mode of life of the animal determine the convolutional pattern of the brain and the relative superficial area of the archipallium to the neopallium.

This is not a startling conclusion for it is in line with what any trained biologist would expect and agrees with observations on other structures of the body, but is opposed to some of the extraordinary theories which have been advanced relative to the brain.

While, as I have just pointed out, it is impossible to make use of functional areas over a large portion of the brain because they are not known, this need not deter us in these studies till other sciences have cleared up these questions. We have proof in the work of Flechsig, Campbell and Broadmann that the fissures are related to many areas of particular histological character and development. This makes it possible, as Professor G. Elliot Smith has pointed out, to substitute histological areas for functional ones, and to make a topographical survey of the cortex in which fissures become boundaries of definite areas instead of being treated simply as morphological structures. While there is still much to be desired in the method, it at least makes use of all of the knowledge at present available and furnishes a means for comparison between brains of individuals and races. These histological areas will no doubt sooner or later be interpreted by physiologists and prove to be functional areas.



BRAIN OF MEXICAN, MALE, AET., 32

It must be remembered that on a surface diagram it is impossible to give a correct idea of the various fields; in the case for example, of the precentral and postcentral areas the bottom of the *sulcus centralis* and not the lip is the boundary. The areas outlined correspond to those laid down by Campbell in his 1905 monograph.

I think that all who have worked on the brain will agree with Doctor Sherrington that the fissures "are not reliable landmarks" and consequently do not have the morphological significance which was, at one time, attributed to them. On the other hand each one, even of the secondary fissures, stands as a definite expression of interaction between circumscribed cortical areas. Doctor Campbell says of the fissures,

> The fields of the cortex dominating the primary and essential functions are all deposited in relation to important fissures.

As remarked above, it seems to me useless to study fissures alone and attempt to record their minute variations, but we may give them a new significance by relating them to histological areas. I have used the method of macroscopic examination of the cortex suggested by Smith as a substitute for the more laborious histological methods and find it quite satisfactory for fresh material. On hardened material, which makes up the bulk of my specimens, it has proven impractical. I prefer then to use the cortical map which Campbell worked out, not only because the work can be checked by histological examination at any time but because fewer areas are involved, which with our present physiological knowledge seems to me to be a decided advantage.

The accompanying illustrations of a Mexican brain, plate xv, are introduced to show our method of making records. Photographs are made of the brain in the usual way, then the brain is very carefully studied and the fissures are traced on the photograph with India ink, lastly the histological areas are marked out. If it is desirable to make a simple line drawing it is only necessary to bleach the completed photograph by one of the usual methods and a black and white line drawing is the result, for the India ink is not affected by the process. I prefer pictures like those in the accompanying plate, for they have all of the advantages of both the usual line drawings and photographs. The plotting of cortical areas furnishes a great aid to the interpretation of fissures and renders the comparison of the development of different areas possible.

This is not the place for recounting details or making comparisons which are at best involved. Any one who is interested in these may review them in our papers which are published elsewhere. I desire to state a few conclusions based on the studies we have made by the method just outlined.

The Negro brains of our series present a very interesting condition in the frontal region. A number of authors have spoken of the ill-filled or underdeveloped frontal region. If we may apply the same reasoning to this region as has been applied to the occipital region, and I see no reason why the process of growth adjustment may not be the same in both, the frontal association area bounded by the superior and inferior frontal sulci may be considered as the site of a variable growth activity. The expansion of the area will push the superior frontal sulcus upward and diminish the prominence of the *sulcus frontalis mesialis*, and, as in the case of the *sulcus occipitalis inferior* of the occipital region, will cause disintegration of the *sulcus fronto-marginalis*. If the development of the frontal association area is slight the converse would be true, that is, the *sulcus fronto-mesialis* would be present and the *sulcus frontalis medius* poorly developed. We have found the latter condition remarkably constant in our series of Negro brains which would suggest poor development of the frontal region or more specifically of the frontal association area. It might be suggested that this condition is due to dolichocephaly, but, judging by a comparison which we have been able to make with a small number of Caucasian brains, the condition is uninfluenced by head shape.

Cunningham thought because the *sulcus frontalis mesialis* is absent in apes and present in man that its simplicity indicated inferiority. We are forced to conclude that it merely represents a human accessory sulcus whose complexity depends on the width of the *gyrus frontalis superior*. The prominence of the *sulcus frontalis mesialis* would seem to indicate, if not a poorly developed brain, at least a lack of development in the frontal association area.

A number of observers have noted the prominent parietal lobe in the Negro brain. This condition was noted in all of our series and particularly in the region of the *lobulus parietalis inferior*, which presents a mushroom-like outgrowth which overlapped the surrounding cortex by operculi. There appears to be also a variation from the accepted type of fissuration in this region. It is undoubtedly an expression of the prominent growth of the inferior parietal region. It is in the nature of one or more accessory fissures situated behind the termination of the fissure of Sylvius. Smith has probably figured it as a dividing sulcus between the inferior parietal areas which he marks "A" and "B."

When we take into consideration all of the relative characteristics of the Negro brain we may conclude that, while it does not necessarily suggest a closer relation to the apes, it is not as highly developed as that of other races observed and is consequently inferior to them.

In the case of the Mexican and Indian brains we have insufficient data to formulate any conclusions on racial characteristics but from the material at hand it seems that they possess all of the elements necessary for higher individual development. On account of the mixture of races the Mexicans do not make the most desirable material for study, but the case of the Indians is quite different. I feel that every effort should be made to secure and preserve brains of this race before the opportunity is lost to science.

Embryological researches have already contributed much to our knowledge and may still prove to be one of the most valuable methods for clearing up brain problems. Cortical functional areas which are hardly suggested by histological differences may be definitely outlined by a careful study of the differences in the time at which myelinization occurs. Not only may the surface of the brain be subdivided in relation to the time of appearance of the medullated fibers, but these fibers can be traced through the subcortical paths and a suggestion of the function of the area be obtained. I fully appreciate the difficulties Flechsig has had in this field and do not suggest embryological research to the exclusion of other methods of investigation but only as an adjuvant to them.

With an improvement in cytological technique it may be possible to learn more of the cortical nerve cells, the time of their appearance and their development. Such facts would add much to the adult histological studies.

The question of the development of the fissures is not settled.

The theory which assumes that cerebral convolutions have their origin in a disproportion of the brain surface to the skull with a resulting reciprocal mechanical influence on various parts of the brain is too generalized for our purpose. The use of the term "mechano-functional" condition may be comprehensive but does not explain the developmental process. I have applied the Weber method, which Scammon used so successfully in the study of the early pancreas, to this problem. While I am not yet in a position to make a positive statement the evidence at hand seems to suggest that a definite growth process in the region of the fissures is a factor in their formation.

It would seem that cerebral anthropology is dependent for its ultimate development on functional localization in the cerebral cortex. Such localization involves so many different factors that we can not hope for a solution of the problem by any one method of research or by any one department, but only by a thorough weighing and assorting of all the evidence contributed by the investigators in all departments. Since anthropology will benefit so largely by the answer to this question her workers should contribute their share toward its solution.

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HAWAIIAN SHARK AUMAKUA

By MARTHA WARREN BECKWITH

DURING a stay of some months in the district of Puna, Hawaii, two years ago, I came across instances of a belief in animal helpers, half god, half human, who utter their counsels through the lips of some medium, who becomes for the moment possessed with their spirit. Such gods are called *aumakua*. They are bound by obedience to their devotee, who becomes their keeper, and their worship, and consequent service, extends to his family and is handed down from generation to generation. Hawaiians are very unready to discuss this belief with foreigners, or even with those of their own race who mingle with foreigners. My interpreter, a part Hawaiian woman educated in foreign schools, told me that her grandmother, although believing devoutly in the phenomena of spirit possession, would never talk with her upon the subject.

On the coast, sharks are the particular object selected for veneration. In the village where I was staying, I quickly discovered that one family of two brothers named *Puhi* (eel) were much feared among their neighbors for their power to transmit disease to any who fell under their displeasure. Consumption was common in the village, and its victims were supposed to have fallen under the malign spell of the *Puhi*, or of the one other sorcerer they feared, the native pastor of the evangelical church. On one occasion the younger *Puhi* made a birthday feast for his son, but upon report that he had been at pains to secure a black animal without a white hair for the underground roasting, in spite of the popularity of such entertainments, not a man, woman or child outside the family, dared appear at the feast for fear of *Puhi*'s sorcery. The man had a wife subject to fits of insanity—a dark heavy-faced woman with some traces of beauty, and sister to two albinos, men of big frame, white skin, light reddish hair, and blue eyes. As they quarreled with their brother-in-law, we never saw them at the coast. "They talk bad of my father," the son explained, who, handsome, bright-eyed and finely formed, nevertheless inherited his mother's mental flightiness.

Under these circumstances it was no great surprise to learn that the *Puhi* had a supernatural helper or *aumakua* who appeared in the form of a particular shark inhabiting the waters off Cape *Kumukahi*. My informant was the native clergyman in the adjoining village of *Kaweleau*, named *Kaiwi*. Said he: "When the *Puhi* go fishing, the shark appears. The *aumakua* obeys the voice of man; name the kind of fish you want and it will bring it. The men give it some of the first catch, then it disappears, and they always come back with full nets. Only when the shark appears do they have luck (hence they recognize the god's intervention). Sometimes the *aumakua* tells them beforehand in a dream that it has gathered the fish together. Besides this, the *Puhi* family can never be drowned. If there is a storm and the boat capsizes, the shark appears and the man rides in on its back."

In answer to questions we drew from *Kaiwi* that this *aumakua* was a particular shark, light with yellow spots, called *Ke-au*. It originated when certain ancestors of *Puhi* had an abortive child.[1] They buried it in the ground, but the *aumakua* came in a dream and told them this was not right, so they threw it into the sea and it became this shark. "How can one tell that a shark is an *aumakua?*" we asked. *Kaiwi* said: "You can tell because when the mother goes in to bathe, the shark will come and jump at the breast as if to suckle; thus she knows it is her child, for it does this to no one else." As *Kaiwi* talked we heard his wife muttering in the back part of the house and presently she called him peremptorily from such dangerous gossip.

We asked the native clergyman of *Puhi*'s own village, a man of higher intelligence than *Kaiwi*, to explain to us more fully the idea of the *aumakua*. "How does the god come to have the shape of a shark?" we asked. "The *aumakua* has no form," he said, "It comes in the shape of a wish into the mother. When she is in trouble she prays and the object comes before her. After the one dearly loved passes away, he is worshiped, the dead one's spirit is fed with *awa* (the intoxicating drink of the Hawaiian). Or if a child comes before its time, unformed, lifeless, such a child is thrown away. The spirit comes back. The mother is then unlike herself—the face is the same but the thought is changed. She says: 'Have you any *awa*? Have you any food?' when such may be before her in the house. The friends will say 'Who are you?' The mother says, 'I am so-and-so'; then the people flock about her and ask the *aumakua* to help them."

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[1] I am told that "Hawaiians suppose an abortive child to be the offspring of an *aumakua* and a human. If an *aumakua* finds the remains of such a child he adopts it, causing it to be like himself, another *aumakua*."

The idea is a simple one. The presence of a spirit is indicated by a divine possession in which the person possessed speaks not as he is accustomed but in the character and with the words of the spirit whose medium he is. His utterances are not his own but are the means by which, together with dream and vision, the spirit of the *aumakua* counsels his protegé.[1] In order that the *aumakua* may be strong enough to act his part as helper, he must receive offerings of prayer, and of sacrifice in the shape of food and drink called "feeding the spirit." For example, a woman living near *Kealakekua* was seen each night to carry a pail from her house to the cliff and empty it over into the sea. It was found that the pail contained *awa* drink which was being fed to the household *aumakua*.

May any creature, plant, or object, then, become an *aumakua*? Logically, yes; but practically there are only certain ones which are regarded as possible god bodies. Mr. J. S. Emerson enumerates these in his paper called "Lesser Hawaiian Gods" published among the papers of the Hawaiian Historical Society. The shark, cowry shell, limpet, squid, and eel are famous sea *aumakua*. The mudhen, plover, chicken, wild-goose, frigate-bird, besides song birds, are important *aumakua*. The spider among insects (not mentioned by Emerson) and the lizard among reptiles are worshiped, and of quadrupeds the pig, dog, and rat.

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[1] Miss Laura Green furnishes me with some notes as to this belief in spirit possession. When the *aumakua* comes into a home where illness or other trouble is present, it enters the body of some member of the family—by the head if a friendly, by the feet if an evil spirit. The person falls asleep, the *aumakua* taking the place of the spirit thus ejected. Such a medium is called a *haka*, and the seance a *noho* (sitting). If a friendly spirit, it offers advice as to how to escape the illness or other troubles—what prayers to offer, fish to catch, herbs to gather. To test the *aumakua*, a member of the family throws a wreath of some special plant (*lai* or *lauhala*) about the neck of the *haka*. If it is a friendly spirit, he will take it, if an evil spirit, he will spit, glare, tear his clothing, or even plunge naked into the bushes. Such insanity is relieved by a drink made from a coarse kind of grass. To keep out these evil spirits, house and yard are sprinkled with water to which salt and a bit of root similar to the ginger are added. This ceremony is also used in case of death.

Rocks, too, are often set up as *aumakua* and worshiped as transformed deities. Certain trees, sweet-scented flowers and phenomena of nature and the elements, the volcano, for example, are powerful *aumakua*.[1] In fact, the *aumakua* worship corresponds well with Polynesian utterances about the creation of the world in which earth, rocks, plants, and animals are imputed to sex birth and their race and lineage recorded. The singling out of special classes of objects perhaps signifies merely that to such names the *aumakua* of famous families of the past got attached and hence their descendants worship these inherited forms.

Are the *aumakua* friendly or evil spirits? Cases differ. Some, says Emerson, such as the shark and limpet, which calm the waves or provide food for their patron, are beneficent; some like the dreaded worm (*enuhe*) and the lizard (*moo*) are much to be feared. The dog is beneficent, but a great thief. The pig is a mischief-maker, symbol of lechery and filth. The rat is, like the owl, beneficent, from the classic tale of the rat-god who, when *Makalii* (Little Eyes—the Pleiades) tied up all the food of mankind in a net and hung it in the heavens, gnawed the ropes and let it tumble back again to earth. Emerson and Fornander[2] both point out the difference between Hawaiian and Tahitian feeling in this respect, the Tahitian *oromatua* being regarded as malevolent beings carefully to be propitiated. In general, the *aumakua* protects its own family, however inimical it may be to strangers.

Are the *aumakua* class or individual gods? In some cases, individuals are worshiped, in others all the species of a class. Emerson says:[1]

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[1] Miss Green says, however, that *Pele*, like the gods and goddesses, is not thought of in the *aumakua* class. Even the pig-god is a superior kind of *aumakua* because of his high lineage.

[2] *Polynesian Race*, vol. 1, page 127.

> In the case of the *pueo* (owl) all birds of that species were equally considered as representative of the *aumakua*, known as "*Pueo nui o kona*" (the great owl of *Kona* district, looked upon by a great portion of *Kona* people as their family god). They were not worshiped as individual owls, and when one died, the life of the *aumakua* was not affected.[2]

On the other hand,

> Each several locality along the coast of the islands had its special patron shark whose name, history, place of abode, and appearance were well known to all frequenters of that coast. Each of these sharks, too, had its *Kahu* (keeper), who was responsible for its care and worship. The office of *Kahu* was hereditary in a particular family and was handed down from parent to child for many generations, or until the family became extinct.

It is to these inherited shark *aumakua* that *Puhi*'s helper belongs. Mr. Emerson has listed a number of similar shark *aumakua* identified along the coasts of various islands, the manuscript notes of which he kindly allows me to print here. For the *Puna* group my informants corroborated, in cases where the name was familiar to them, the story set down some thirty years ago by Mr. Emerson. At the name of *Kupanihi*, for example, a wooden-legged man of the party, named *Moses Kuaana*, spoke up. "That is my shark," he said. "He puts a person under his arm; he is like a father to me." *Moses* told me that his shark was rainbow-colored, that it was man before and became shark, and that it acted as protector to his family. Another bystander recalled the story of the abortive child of *Aia* of *Palama* (Emerson's *Ahia*) which was thrown into the sea. Its spirit came to the grandmother and told her it was living and wanted *awa* to drink, warning her to attend to its message lest one of the family die. This proves that the data not only preserve the tales of old and mythical shark gods, but represent a living faith to-day.

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[1] *Papers of the Hawaiian Historical Society*, no. 2, page 8.

[2] But one informant tells me that the owl is classed as a god, not as an *aumakua*. According to old Hawaiian ceremonial, the *opelu* and *aku* fish, excellent for eating, were held tabu each for six months at a time. On January 13 was performed a religious ceremony accompanied by human sacrifice, at which the *aku* season closed and the *opelu* began. This commemorated the voyage of the priest *Paao* from *Tahiti* with chief *Pili*, when schools of *aku* and *opelu* accompanied the canoe all the way to still the waves. See Malo, *Hawaiian Antiquities*, pages 25, 199.

From Mr. Emerson's notes we learn something of the form in which the belief survives. Shark gods may be male or female. Those described are invariably red, shining, light or spotted to correspond with their sacred character, as allied to the gods. They are of human origin, the constant reference of shark or lizard gods to an abortive child being possibly suggested by the appearance of the partly formed foetus. Their worship is handed down from father to son, a special keeper (*Kahu*) being intrusted with their care. They are invoked with particular prayers and have temples erected for their worship. Their special function is to aid in the food supply of the household—generally by giving the fisherman good luck at sea—and to protect him from drowning. They are, in fact, regarded as spirits of half-human beings which, rendered strong by prayer and sacrifice, take up their abode in some shark body and act as supernatural counselors to their kin, who accordingly honor them as household divinities.

In most cases the *aumakua* has the power to take either human or animal shape. Most old Hawaiian hero tales are based upon the exploits of some supernatural being born as a rope, a chicken, or in some abortive form, cast out to die, but, being nurtured by a seer, survives to avenge family wrongs at the hands of well-nigh invincible foes, to furnish culture gifts, or undertake strange adventures. Hence follow a number of transformation stories, of which the tale of the pig-god, *Kamapua'a*, and of the shark *Kamoho-alii*, brother to *Pele* of the volcano, are the most popular survivals. Says Emerson of the latter:

> His place of abode on Hawaii was *Ka-pali-kapu-o-kamohoalii* ("The sacred steep of Kamahoalii") on the northern edge of the crater of *Kilauea* on Hawaii. So awful and sacred was this spot that *Pele*, though his own sister, dared not allow the smoke from her fires to rest upon it.

His girdle too was of red, corresponding to the sacred color ascribed to supernatural sharks.

A popular form of the transformation story is that of the shark-man born with a shark's mouth on his back and the power to become a shark the moment he leaps into the sea.[1] This tale, four variants of which Emerson has collected on Hawaii, resembles the stories of robbers or man-eaters that waylay unsuspecting travelers who pass by on the highway at some narrow passage. Two such spots I have seen, one an upland pass on Oahu between two gorges where *Aikanaka* lay in wait for his cannibal feasts,[2] the other in *Puna*, where a high cliff presses upon the shore road. Here the natives still point out the mass of stones hurled in the final struggle with the dreaded highwayman *Alohi*.

In Emerson's first version, the shark-man (unnamed)

> lived in *Kawaiuhu* at *Kaalualu*, *Kau* district. He had the form of a man with a mouth on his back. He was a great farmer and when people went to *Kawaiuhu*, he resumed his shark form and devoured them. To destroy him the chief ordered all the men to assemble on a given day and cultivate the ground. A *makaula* (prophet) discovered this shark-man among them by his concealing his back with a *'pau* (strip of bark cloth). By the prophet's orders, the monster was seized before he could reach the sea and resume his shark form. They killed him and burned him up.

Portions of this shark, says Emerson, were cooked in the ovens (now open) of *Kaluapuali*, *Papalaheiau*, *Palalauhala*, *Mohoiki*, *Kahiu*. The oven *Puuolokuana*, however, remains unopened, for *Kane* and *Kanaloa* and the other gods were cooking the shark there when the day dawned, at which time gods cease their activities on earth.

The second version comes from *Waipio* valley.

> *Nenewe* lived beside the large basin at the bottom of the water-fall on the west side of *Waipio* valley. As he cultivated his little sweet potato patch he was in the habit of watching the passers-by, and sometimes called out to them "Where are you going?" Perhaps the answer would come "We are going to bathe in the sea at *Muli-wai*" in which case he might answer, "Then look out that the shark there does not eat you." He would then rapidly get over the intervening space of about two miles, and, as a shark, await their coming at the seashore. In due time he would seize and devour one of their number. When this had happened several times, people began to suspect the fellow at the potato patch of being in some way connected with the shark of *Muli-wai*. They accordingly seized him one day at his home, and discovered the shark's mouth under the *kihei* (cape) which he always wore on land.

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[1] See Thrum's *Hawaiian Folk Tales*, page 255.

[2] Compare *Ibid.*, page 139.

In a third version corresponding with the above in almost every particular, the shark is named *Niho-kahi* (single tooth) and lives at *Laie*, on the north coast of Oahu.[1]

A fourth occurs under the name *Ka-ai-poo* (the head-eater). He lives at *Kapaahu*, *Puna*, and carries the mark of the shark's mouth on his back.

Hawaiians often think of their *aumakua* as going to war among themselves.[2] A second famous shark myth detailed by Emerson is that of the great shark war, in the course of which the man-eating sharks were driven from the group. To this event belong the most ancient and formidable gods described in the notes. Some details of the war point to a symbolic rendering of a real struggle between chiefs, but whether of a literal struggle to weed out cannibalism from the group is doubtful. There is no evidence that cannibalism was ever practised in Hawaii, nor, says Fornander, in the Society Islands. Among the Tongans the practice is said to be exceptional. But in the Fiji and the Marquesas, in New Zealand, to some extent in Samoa, the custom prevailed. The cannibal rite must therefore at various periods in the settling of the group have forced a decision. It is, however, more in accordance with Hawaiian symbolic analogy to employ the figure of the man-eater to represent a great war chief, who "eats" the land or his rivals in war, than in the more obvious analogy of the cannibal rite.[3]

The story itself includes so many inconsistencies that it is hardly worth while in this paper to enter into its detail. I will merely enumerate the sharks named by Emerson, and indicate their place in the legend.

Kaahupahau (w)[4] and her brother (or son) ***Kahi'uka***, live in the ***Ewa*** lagoon off Oahu. She is in one version the friendly deity of the ***Ewa*** natives, who herself instigates the war against the man-eaters,[1] in another,[2] one of the man-eaters and slain by the hero *Kealiikana.*

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[1] Collected April 10, 1907, from J. A. Kahiona, a mormon of Laia.

[2] Compare the story of the great owl war, Thrum's *Hawaiian Folk Tales*, page 200.

[3] At one place in the story the hero shark and his companions meet a man-eater. "They engaged him in conversation, when it appeared that he ate human flesh. They told him it was wrong, to which he replied that it was nice and that they must defeat him before he would stop doing so." In another passage "the spirit of the man-eater revived again, and as a tongue, now restored and alive, made its way to the coasts of Maui and Hawaii, pleading with the sharks of those regions for vengeance."

[4] k stands for male (*kane*) w for female (*wahine*).

Kuheimoana (w) is the greatest of all sharks. She lives at *Kaula* and is thirty fathoms long. Subdued by the *Kau* shark-god, she joins his party, but being mistrusted, is led into shoal water where she is stranded, and decides to return home.

Kupiopio (k) is her son, seven fathoms long, who lives at *Keamoku, Haena.* He is slain by the *Kau* shark-god. According to Emerson's notes "He came from *Kaula* to live at *Hamakua.* On his way he fell in with *Makaluahau* (k) of *Kalihi,* Oahu, who became his *aikane,* and went with him to *Hamakua.* There their spirits rested and directed the people of *Paauhau* to plant *awa,* which their keeper brought to feed them."

Kealiikaua (k) is the hero of the great shark war. He is born at *Ninole, Kau,* on Hawaii. He acts as the friend of man, his great work being to travel about the islands and slay all those sharks who feed on human flesh. Four companions accompany him.

Kalani (k) is "born on the coast of *Waiohinu* (*Kau* district) from the eye of his mother. His blood has been seen on the forehead of some who worshiped him. He guarded all the people of *Kau* from the other sharks who might harm them. He went to the great shark war at *Ewa,* Oahu, with his kinsman and friend *Kaholeakane.* They were swallowed up by *Kuheimoana* in this war. The little *Kalani* went first into the mouth of the monster, followed by his larger friend, whose size forced the monster to disgorge him. As he came out, the nimble *Kalani* darted out too. Then they swam into shoal water and thus led *Kuheimoana* to her fate. She got stranded on a shoal and was kept from the battle. *Kalani* went too near the shore and had a portion of flesh cut from his back by the people of *Ewa,* who ate it." In another version, two pieces of his flesh form the spouting horn at *Kealae.* The natives say "If a man in a canoe wears anything red, *Kalani* will pursue the canoe and upset it."

Kaholiakane (k) is companion to *Kalani* in the great shark war, *Kua* (k) a *Kau* shark who joins *Kalani* in the shark war, *Kane* (k) companion to *Kalani.*

Ke-alii-holo-i-ka-moana (the chief sailing over the ocean) (k) lives off *Kekaha, Puna,* from *Ka-lai-o-kawili* in *Apua* district to *Ka-lai-o-wili-ea* in *Panau-nui.* "He began life a human child living on land, was a *kaukau-alii* (low chief) under *Iwakakaoloa,* the blind chief of *Puna.* He was an expert fisherman, frequenting the sea in a canoe. At death, wrapped in *Kapa-ahu-na'lii,* he was cast into the sea at *Kealakomo* and became a shark-god of the class called *akua-noho* who were supposed to 'dwell with or be over men as guardians.' He showed his friendship to men by warning them of the approach of hostile sharks by exhibiting himself above the level of the sea. He went with the other sharks to the great shark war at Oahu."

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[1] *Papers of the Hawaiian Historical Society,* no. 2, page 10.

[2] Collected March, 1888, from *Kuamuamu,* and dictated by Mr. Emerson to H. E. M. Alexander.

Malu'o[1] (k) lived with *Ke-alii-holo-moana* and went with him to the war.

Ka-pani-la[2] (the shutting out of the sun) (k) is so-named "because his enormous bulk would obscure the sun should he come to land. He is the largest of the sharks. His usual haunts extended from the point *Ka-lae-o-lamaulu* in *Kapoho, Puna,* to *Kumukahi* point in *Kapele.*" He was friendly to the natives and "great things were expected of him when he led the Hawaiian sharks to war, but off Diamond Head he got stranded in shallow water and could proceed no further."

Besides these mythical and fabulous beings who are famous in the great shark war, Emerson lists a number of well-known shark-gods worshiped at various points about the group:

1. *Hika-welo-ula*[3] (k) of *Puna,* Hawaii, son of the *Kau* shark, *Ke-alii-kaua* and of *Ahia,* a woman of *Kalapana.* At birth he was covered with red tapa, the kind called *pukohukohu,* and became a red shark. He had two forms, that of a shark at sea, that of a man on land.
2. *Kane-i-kaupaku*[4] (k) of *Puna,* Hawaii.
3. *Kane-mahuna*[4] (k) of *Puna,* Hawaii.
4. *Ke-au* (k) *Puhi's* shark off *Kumukahi,* Hawaii.
5. *Haloa,* a shark of *Mahana, Kau,* who comes in his spirit form and teaches his *kahu* the medicine to use to cure diseases.
6. *Humeke,* of *Kaalualu, Kau.*
7. *Mikololo,*[5] of *Pokini, Malilele* cliff, *Kau.*
8. *Pai.*
9. *Uukanipo* ("the *uu* that sounds at night"), two great sharks of *Kawaihae* in *Kohala,* twin brothers. They lived at *Kamani,* and were regularly fed with *awa.* When the king wished to see them the keeper hung two bowls of *awa* from a forked stick, and each shark drank from his own bowl and went away.
10. *Ohiki,*[6] a stone image of a shark at *Kapaaheo,* in *Puuepa, Kohala,* near the coast. It is about eight feet long and very heavy, and was the great god of Kamehameha I. It is situated a quarter of a mile south of the temple of *Mookini.*
11. *Hikeaweaweula,*[7] a shark who lives in a cave called *Ke-ana-o-hikeaweaweula* off *Kahulaana pali, Kohala.*

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[1] Collected March, 1888.

[2] Collected December, 1892.

[3] Collected April 2, 1886.

[4] Collected March, 1888.

[5] Collected March 16, 1907, from W. P. Kanealii.

[6] Collected in 1888. Such images occur at many a good fishing ground along the coast, the shark being preëminently the fisherman's god. In a brackish pool on the coast below *Hilea,* Hawaii, I was shown such a rock. It lay on its side in water up to the waist, somewhat resembling a fish in shape, and was one of a pair once highly prized as gods who brought the fish; but a recent earthquake had destroyed the natural sluiceway up which the fish were wont to be driven, and the god's mate had since been sold for a couple of dollars to an enterprising collector.

[7] Collected in 1885.

12. *Hiu*[1] (w) (tail of a fish), listed among both *Kohala* and *Maui* sharks.

13. *Ka-ahu-kolu*[1] (k) of *Kohala*.

14. *Luahine*[1] (w) (old woman) of *Kohala*.

15. *Malihine*[1] (w) (stranger) of *Kohala*.

16. *Moe-kikoo*[1] (w) of *Kohala*.

17. *Na-kiaha* (k) (the drinking cups) of *Kohala*.

18. *Kaahupahau* (k), a shark born of human parents and living along the coast of *Kohala*. Emerson has seen it, and talked with the son of its late keeper. He quotes the prayer addressed to it.

19. *Kaaipai* (k), living at *Kawaihae*, Hawaii. *Keiwi* told me his story as follows: A couple lived at *Puako*, in *Kawaihae*. People from *Maui* used to bring food to *Puako* because the taro plant did not grow there. The shark would go out, capsize the boat, swallow the food and make off to his cave, where he would deposit it. Then he would go in a dream to his protegés and tell them where to find it. Emerson quotes the prayer by which the couple invoked him to capsize the boats when they needed food.

20. *Maonalaau* (k) of *Kohala*, born of human parents, *Koahou* and *Kaiwi*, now dead. In time of famine he sends a great turtle to "the lands of *Niulii*, *Waiapuka*, and *Auau*." The invocation is as follows: "O *Maonalaau*, listen to *Nihoeleki*, *Nihoeleki* of *Kahiki*, the ancient one. Hear this cry. The children suffer with hunger. Behold the food; bring the turtle."

21. *Pehu*[2] (k) (swollen), listed for *Hamakua*, Hawaii, and said to have eaten some natives there, also listed second among the great sharks of *Maui*.

22–26. *Mahiki*[3] (k), *Kawaiiki* (w), *Kaahu* (w), *Kai* (w), *Uhanui* (k), sharks of *Hamakua*, Hawaii.

27. *Ka-ili-pulepule*[4] (the spotted skin) of *Kekaha*, Hawaii.

28. *Kawelo* (k) off *Kuaokaiula* Point, ten fathoms long, friendly to man, but avenges any ceremonial lapse with disease and trouble.

29–31. *Kane-hamama*[5] (kane of the open mouth), *Kolo-pulepule* (spotted creeper), and *Liholiho* (very hot), all of North *Kona*, Hawaii.

32. *Likiliki-muumuu*[6] (*Likiliki* the maimed), the shark-queen of *Kona*, who had lost one of her fins. She was very friendly to the natives and made circuit of *Kona* every year.

33. *Moanaliha*,[7] chief of all the sharks of *Maui*, also ascribed to North *Kona*, Hawaii.

34. *Kane-i-kokala* (k) of *Mahikinui*, *Maui*.

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[1] Collected December, 1892, and March, 1907 from *Kaiooni*, church elder and expert fisherman at *Kahei*, *Kohala*.

[2] Collected April 10, 1907.

[3] Collected December 19, 1892, from *Kaiooni*.

[4] Collected in 1885.

[5] Collected April 20, 1886.

[6] Collected March 28, 1886.

[7] Collected April 2, 1907.

35. *Kaala-miki-hau* (k) of *Maui*, whose invocation reads as follows:[1]

Eia ka ai	Here is food.
Eia ka ia	Here is fish.
Eia ke kapa	Here is cloth.
Nou e Kaala-miki-hau	For you, O Kaala-miki-hau.
Nana ia'u kau pulapula	Look upon me, your worshiper.
I mahiai	Let me plant.
I lawaia	Let me fish.
Kuku kapa	Beat the tapa bark—
A e ola ia'u, Kanui	Grant me life, Mighty one.

36. *Mano-ki'u-wiwi* (shark with a slender tail) of Oahu.

37. *Nui-kala*, formerly of *Loko Uko'a* in *Waialua*, Oahu.

38. *Puhi-ula*[2] (red eel), associated with *Lani-wahine* and living at *Loko Uko'a*.

39. *Lani-wahine* (her highness), said to have a temple for her worship in *Waialua*, Oahu.

40. *Kala-kiki*[3] (k) of Waialua, who is worshiped in a *heiau* (temple) called *Ka-heiau-o-Kalakiki* just above *Kaupakuhele* on the west side of *Makaleha* valley.

41. *Na-naulu* (k), also of *Waialua* and worshiped in the temple called *Ka-heiau-o-kaunu* north of *Kala-kiki's* temple.

Such is the form which the shark worship takes in Hawaii. What is the exact nature of the *aumakua* belief? The ancient Hawaiians worshiped three classes of deities, *akua*, *aumakua*, and *unihipili*. *Akua* were thought of as spirits only, not born of humans. *Aumakua* were the offspring of god and human, or those human beings who were marked by an abnormal shape which might be assumed at will, such as that of some animal or object. *Unihipili* were the spirits of departed relatives. To prepare an *unihipili* the flesh was stripped from the body and the bones wrapped in tapa and mats, or sewed into a woven case which took the form of the body. Such a god, if properly worshiped, was bound to fulfill the requests of its devotee. Both *aumakua* and *unihipili* belong to the class of *akua noho* or "gods indwelling," that is, of those spirits who enter into and possess human beings, through whom their messages to their devotees are uttered.[4]

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[1] Recited by *Mahelona* of *Puuhale*, Maui.

[2] Collected in 1888.

[3] Collected March 16, 1907, of Barenaba.

[4] For this classification I am indebted to Miss Laura Green, social worker among the Hawaiians of Honolulu, whose informant was an intelligent Hawaiian woman well versed in native custom. See also Malo, *Hawaiian Antiquities*, pages 142, 155–158, and Emerson in *Hawaiian Historical Papers*, no. 2.

The idea of the *aumakua* in the Hawaiian theogony seems to be that of enforced helpfulness within a kinship group as a means to insure superhuman coöperation in individual human affairs; individual, because the *akua* is a national god, to be invoked in time of war or national calamity. The *aumakua* interests himself in providing food for the particular family to whose protection he is devoted, in curing disease, or avenging an enemy. For all such personal services, says Miss Green's informant, the sufferer fears to invoke a national god, lest he be angry; he therefore calls upon his lesser gods. We find the *aumakua*, therefore, closely concerned with affairs of sorcery, and his keeper looked upon as a magician of malignant power. Malo scoffs at the claims of the *aumakua* keepers, whom he accuses of duping the people by pretending to speak as if inspired by the god.

Nor is the *aumakua*, like the *unihipili*, subject to a particular keeper only. Emerson makes the ancestral, family character of the *aumakua* his distinguishing feature from the *unihipili*. He serves the whole family, and when one keeper dies, another takes his place.

For although worshiped like a god, the *aumakua* is nevertheless ranked as *kauwa*, or of the servant class, because bound to obey those whom he serves. He may be compared with the Arabian genii whose supernatural assistance is forced through the possession of some special talisman. In the case of the Hawaiian *aumakua*, that talisman seems to be the social fact of kinship, the act of invocation perhaps serving as a charm to enforce service. This same enforced obedience characterizes the relation between the *unihipili* and his keeper. "The dead are honored by being laid away in caves," says Miss Green's informant. "By being placed above ground they are humbled in their own eyes, as well as in the eyes of those who see their *pu-o-lo* (package form). Hence their implicit obedience to the will of the priests. They are truly *kauwa*, of the lowest class."

Malo, in a curious chapter, which Emerson tells us has undergone mutilation, applies the strict laws of descent to the *kauwa* class. He says:

> Those who were *kauwa* to their chiefs and kings in old times continued to be *kauwa*, and their descendants after them to the latest generation; also the descendants of the kings and chiefs, their masters, retained to the latest generation their position of masters. It was for this reason they were called *aumakua*, the meaning of which is ancient servant—*kauwa kahiki*. They were also called *akua*, *i. e.*, superhuman or god-like, from some superstitious notion regarding their power.[1]

Now according to Polynesian habits of analogy, we should look for the prototype of such a supernatural relationship as that involved between an *aumakua* and its keeper in the social organization itself. Malo himself, if we interpret his words correctly, asserts a close relation between the *kauwa* and the *aumakua*. It is possible to assume that *Wakea's kauwa* may have been an *aumakua*, and *Papa's* liaison was with that supernatural demi-god whose keeper and master *Wakea* was. Such relations with a *kauwa* were in Malo's day, as they still are, considered highly disgraceful and for this reason chiefs of pure blood were closely guarded to escape such a connection. According to Malo, members of the class bore distinct tatoo marks—a dot or V-shape over the eyes, or curved lines on either side of the temples—which recalls Mr. J. S. Emerson's note on the shark god *Kalani* "His blood has been seen on the forehead of some who worshiped him." These things point to a direct relation between *kauwa* and *aumakua*.

The whole matter, however, remains still obscure. Light may be thrown upon it by comparing the Hawaiian *kauwa* with the Fijian *tauvu* relation described by Mr. Basil Thomson in *The Fijians*. Thomson explains the meaning of the word as "sprung from the same root" or "of common origin." He finds the *tauvu* relation existing between tribes "who may live in different islands, speak different dialects and have, in short, nothing in common but their god." He says:

> A tribe never forgets the tribe with which it is *tauvu*. Members of this tribe may run riot in its village, slaughter its animals and ravage its plantations, while

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[1] The *kauwa* class Malo derives from the *kauwa* of *Wakea* (ancestor of the Hawaiian race) whom his wife *Papa* took for her husband after *Wakea's* death. This seems to have occurred elsewhere than in Hawaii, for not until the fifth generation from *Papa* and her *kauwa* husband does Malo trace the "actual and real *kauwa* of Hawaii." Malo, *Hawaiian Antiquities*, pages 96–101.

> it sits smiling by: for the spoilers are its brothers, worshipers of its common ancestors, and are entitled in the fullest sense to the freedom of the city.

On the other hand, in case of war, *tauvu* tribes aid the warring chief. Thomson explains the relation as due to intermarriage at a distance, but one is inclined to suspect the presence of the *aumakua* idea, that is, of a union between a human and a god, especially since one origin tale tells how the ancestress of a *tauvu* family in the village of *Nambundra*, while in bathing, was seized and brought thither by a monster shark, god of that place. The individual relation between keeper and *aumakua* may have here been generalized, and the master tribe be entitled to make free with the property of the *aumakua* or servant tribe, or summon its aid in war, as a result of kinship between them.

In short, given the abortive or premature foetus forms, which the primitive mind might most readily explain by supernatural beast unions, and under the stress of Polynesian preoccupation with ideas of kinship, rank, and inheritance, the concept might emerge of an *aumakua* or supernatural helper, part beast, part human, and bound to the service of the family as *kauwa* to his chief. Further investigation is, however, required to establish the historical relation between the *kauwa* and the *aumakua* ideas in Hawaii. We may be looking for a correspondence where the primitive mind sees only an analogy.

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THE AMERICAN ZODIAC

By STANSBURY HAGAR

IN a recent article Dr. Herbert J. Spinden states his conviction that, while the existence of a native zodiac in Central America is an open question, evidence relating to it is practically undeveloped, and the age-long fallacy of pre-Columbian introduction from Europe or apparently from anywhere else outside of America, should be dismissed.[1] He also criticises many of my papers on the subject and also those of other writers.

As the late Mark Twain wrote, it is difference of opinion that makes horse-racing, and the same cause impels me to present my opposing views for the consideration of students. I wish to establish truth, whether my present view of it be right or wrong. For many years in the *Anthropologist* and the publications of the International Congress of Americanists, I have asserted from conviction the existence of a native zodiac in America which presents a very similar aspect in Peru, Yucatan, Mexico, and among the Pueblo tribes of our own country. The existence of this zodiac is, of course, entirely independent of the question as to whether it has been derived or influenced from the eastern continent. One can accept its existence in America and deny oriental influence. My conviction is, however, that the analogies between the oriental zodiacs and the various forms of American zodiacs are too arbitrary and complex to be satisfactorily explained otherwise than by intercommunication. But I have carefully refrained from pretending to any knowledge as to when, how, or where, intercommunication occurred beyond the limitation that it was accidental and sporadic, not the result of any general migration, and that it took place at least many centuries prior to the Spanish conquest.[2]

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[1] "The Question of the Zodiac in America," *American Anthropologist* (N.S.), vol. 18, pp. 53–80. Subsequent references to pages included in parentheses in the text are to this paper.

[2] Hagar, *American Anthropologist* (N.S.), vol. 14, pp. 43–48.

Therefore, I have not eliminated the possibility of communication from western Europe nor have I insisted upon the Chinese junk or the Phoenician galley which seem to disturb Dr. Spinden's equanimity, although I regard them as strong possibilities (p. 56). In answering Dr. Spinden's criticism of my papers I will not deceive myself with the belief that I can hurl thunderous phrases or heavy projectiles of opinion from the impregnable cliffs of authority and infallibility upon the forlorn hope of those attacking my views from below. On the contrary I welcome sincere criticism which seeks to establish truth by understanding and justly appreciating an opponent's views, trying to state them as that opponent would state them, and then pointing out those errors, whether of principle or detail, into which every student in a new field is liable to fall.

First, as to the existence of a zodiac in America. Perhaps the most important element in the structure of these studies is sequence. Evidence relating to the signs of the zodiac is usually found not here as to one sign and there as to another, but in direct continuous and complete sequence, occurring in many different forms of art, architecture, ceremonials, and calendar divisions. The only exceptions in the entire field are the more complex sequences of the Peruvian star chart of Salcamayhua and the plan of Teotihuacan. The identification of any two signs in one of these sequences absolutely determines the position of all the others. The symbols of all must then be consistent in the positions indicated or the identification fails. Also wherever the existence of a zodiac is suspected its entire series of symbols must harmonize throughout in continuous and unvaried sequence with the sequence of other representations of the zodiac. The harmony must be complete between the different series as a whole and between at least many of their corresponding parts. Surely it must be apparent that under such a rigid limitation it is highly improbable that a zodiacal sequence can be constructed by chance or imagination or manipulation in any form. It can only exist because it was designed to exist by some human mind. Conversely every zodiacal sequence found adds immeasurably to the improbability of accident or imaginative manipulation.

It is not possible to justify in detail the writer's claim to have shown such sequences within the limits of this paper. He can only refer to his published papers and give here the results explained in them.

In Peru the evidence is based upon five principal sequences. First there is the star chart of Salcamayhua, an Aymara or Colla Indian who wrote about 1613 "to show what these heathens thought." He is generally regarded as a reliable authority. On this chart the signs of the zodiac are depicted in three vertical columns of four signs each, every sign being three signs distant from its neighbor vertically and four signs distant horizontally.[1] Next there are the twelve wards of Cuzco arranged in a circle around the sacred city of the Inca and each bearing the name of a zodiacal sign. The purpose was to imitate upon earth the celestial design of the sky and thereby to bring down some of the perfection of the celestial world. The Skidi Pawnee of our own country today exhibit a similar concept in the arrangement of the village groups of their tribal encampment so that their relative position corresponds with the relative position of the asterisms to which their shrines are dedicated.[2] These wards of Cuzco are named and located by Garcilasso de la Vega, and, in later times, by Squier and Wiener from undoubtedly ancient tradition. Many of them are mentioned in connection with early events in the city's tradition or history.

Within Cuzco were eleven districts which also bore the names of zodiacal signs, one district having apparently been forgotten. They are mentioned by the same authorities and are arranged in a continuous meander.[3] In the outskirts of Cuzco were groups of shrines divided into ceques, so called, which also reveal zodiacal influence. Finally, the ritual of the twelve monthly festivals celebrated in Cuzco and throughout the Inca dominion presents a zodiacal sequence which indicates that the rites of each festival were directed to the sign through which the sun was passing at the time the festival was held. This ritual is described with minor variations by almost all the early writers on Peru.[1] Blas Valera, probably the best authority on ancient Peru, writes that the Peruvians gave different powers to their zodiacal signs such as the llama, puma, serpent, and plants. Each asterism was believed to have the form of the animal or object that it governed and images were made of all these constellations. There was a temple in the Amaru Caucha district of Cuzco dedicated to the sign Scorpio in the form of a serpent or dragon holding a scorpion in its mouth.[2] Calancha also mentions the Puma asterism as a sign of the Peruvian zodiac and identifies it with our Leo. Other early writers upon Peru refer to several of the constellations included in the various sequences but seldom attempt to identify them. As yet the writer has been able to publish only an outline of the evidence bearing upon the Peruvian zodiac, but the harmony of the Peruvian sequences backed by these authorities should suffice to establish its existence.

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[1] Hagar, *International Congress of Americanists*, XII *Session*, Paris, 1900, pp. 271–284.

[2] Fletcher, Alice C., *American Anthropologist* (N.S.), vol. 4, pp. 731–733; Dorsey, G. A., "Traditions of the Skidi Pawnee," *Memoirs, American Folk-Lore Society*, vol. 8.

[3] Hagar, *International Congress of Americanists*, XIII *Session*, New York, 1902, pp. 217–225.

In Yucatan the zodiacal sequences comprise drawings in the Dresden and other codices, mural inscriptions at Acanceh and Santa Rita, celestially planned cities at Uxmal and Izamal, a vigesimal division of the zodiac among the twenty Maya days of the uinal period or month and the ritual of the festivals divided among the eighteen uinals of the year.[3] The sequence is simple, direct, and practically unvaried in all cases, and here we depend much less upon tradition than in Peru because the symbols of the codices and inscriptions and the glyphs of the uinals and days provide impressive testimony independent of external authority. Yet the three oldest and best authorities upon Maya matters, Landa, Lizana, and Cogolludo, contribute their share to the evidence. The explanation of Uxmal is partially dependent upon it; Izamal almost wholly so. As in Peru, the pre-columbian age of all the sequences, and their freedom from later European influence cannot be questioned. No early writer either asserts or denies the existence of a Maya or Mexican zodiac, but a number of symbols are known to represent asterisms, some of them zodiacal. Below the Acanceh inscription is a series of symbols now generally recognized as those of the planet Venus. Seler interprets others of the same planet on a constellation band at Chichen Itza as representing that planet in conjunction with a series of asterisms which must be zodiacal since planets travel only through the zodiac.[1] Spinden refers to another probably zodiacal sequence from the Codex Peresianus (p. 77). Excluding from these two sequences the symbols of Venus, the moon, and the peccary, all the other symbols have already been identified by me with definite zodiacal signs. Again, the ecliptic itself is represented as a rope with its northward and southward course correctly depicted among the symbols of the signs at Mitla, Santa Rita, and on sheet 19 of the Codex Cortesianus.

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[1] Hagar, *International Congress of Americanists*, XIV *Session*, Stuttgart, 1904, pp. 593–602.

[2] *Relacion Anonima in Tres Relaciones de Antiguedades Peruanes*, M. J. de la Espada, Madrid, 1879.

[3] Hagar, *International Congress of Americanists*, XVI *Session*, Vienna, 1908, pp. 277–300; XVII *Session*, Mexico, 1910, pp. 140–159; XIX *Session*, Washington, 1915; *American Anthropologist* (N.S.), vol. 15, pp. 16–32; vol. 16, pp. 88–95.

The lack of direct mention of the zodiac by early writers may be accounted for by its sacred and esoteric nature. Knowledge of it was probably always confined to the few highly instructed ones, and they exhibited little longing to teach their intolerant Spanish conquerors.

In Mexico there are numerous zodiacal sequences in the native codices, perhaps the most important being those in the Borgiano, Bologna, Vaticanus 3773, and Laud. In the three first named the governing deities of the six signs above hurl spears at the deities of the six signs below, hence the group is known as the spear throwers. Most important are the drawings and descriptions of the zodiacal asterisms in their proper sequence in the writings of Sahagun, Tezozomoc and Duran. The two former named five single and one double sign, hence six signs in all, which the Mexican king is supposed to rise at midnight to observe. They positively identify the Mexican scorpion asterism with our Scorpio. Duran calls his asterisms planets, but as all are associated with one of the twenty-day months, they evidently pertain to the zodiac. These asterisms outnumber the planets and no planet can bear any uniform continuous relation with any month. The eighteen Mexican months and their festivals, the twenty days of the month, the celestial plan of Teotihuacan and the Zapotec mural paintings at Mitla also reveal zodiacal sequences.[1]

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[1] *International Congress of Americanists*, XVI *Session*, pp. 150–239.

Among the Pueblo the monthly ritual exhibits an unmistakable zodiacal sequence, the existence of which has been definitely proven by the identification of several signs by the Hopi for me at the First Mesa in September, 1915, and the independent statements of several Hopi to me that all their rites honor the stars through which the sun is passing at the time they are performed. This adds to the testimony of archaeology and tradition the evidence of living Indians who still perform the zodiacal ritual.

Very likely there are a number of other zodiacal sequences in America which the author has not encountered, but, using the material described, he would ask: Independent of its meaning, has not the existence of an analogous sequence of symbols been shown in Peru, Yucatan, Mexico, and among the Pueblo, pervading the most important concepts of art, religion, architecture, and rites? Can this sequence be explained on any basis other than the zodiacal? Is the zodiacal interpretation of it unsatisfactory?

I will now try to answer Dr. Spinden's criticisms. In general he asserts that no first-rate authorities among the early writers on Mexico or Peru favor the existence of a native zodiac in America (p. 53). This is incorrect, as shown above. But a native zodiac could not have existed in Peru, because Garcilasso de la Vega, "our best authority on the civilization of the Inca" denies it (p. 70). Dr. Spinden apparently is not aware that the Anonymous Jesuit, who asserts the existence of the Peruvian zodiac, has recently been identified with Blas Valera, from whom Garcilasso quotes extensively, and with expressions of great respect for his knowledge and learning. There is indeed a strong suspicion that Garcilasso's work is largely based on the lost work of Blas Valera, of which the Anonymous Jesuit is but a fragment. This identification was made by Dr. Gonzalez de la Rosa and was accepted by Sir Clements R. Markham. The latter describes Blas Valera as by far the greatest of clerical writers upon Inca civilization and as having qualifications and advantages possessed by no other writer. Both he and Garcilasso were half-blood Quichua, but the former wrote in Peru. His writings show an intimate acquaintance with the arcana of Peruvian religion and knowledge. Garcilasso went to Spain at the age of twenty and never returned to Perü. He was then too young to have mastered the sacred things of the Inca and he did not write of Peru till thirty-six years later, so that his negative statements are far from certain. Blas Valera rather than Garcilasso is entitled to be called the best authority on any subject he mentions.[1] Salcamayhua, a pure-blood Aymara or Colla, wrote in 1613 what Dr. Spinden terms a crude seventeenth century manuscript supported only by some priestly writings (Blas Valera and Calancha) (p. 69). But Acosta, a Spanish priest who published in 1590, is quoted with approval (p. 70). On the Salcamayhua chart I identify four signs by roundabout arguments and then assume that the rest fill out the zodiac (p. 69). Being tied down by the sequence throughout I cannot assume anything.

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[1] Hagar, *International Congress of Americanists*, XVI *Session*, Vienna, 1908, pp. 277–300; XVII *Session*, Mexico, 1910, pp. 140–172; *American Anthropologist* (N.S.), vol. 13, pp. 229–234.

Because of that sequence it does not matter at what point I begin the argument. The choice is purely personal. We know nothing more of the Puma star than that it is somewhere in the heavens (p. 69). It is identified with Leo by Calancha and with a sign of the Peruvian zodiac by Blas Valera as previously stated.

The "harvest" asterism cannot be the Pleiades because they are also called "the granary." It is ridiculous to suppose that the "crude sketch" of this asterism can represent both a cloud and an ear of maize (p. 69). It is not unusual for the American Indians to delight in seeing more than one meaning in a single form; for instance, the widespread use of the castellated terrace to represent both clouds and mountains and the translation of Quetzalcoatl as both Bird Serpent and Celestial Twin. The Aquarius asterism is identified by means of Assyrian symbolism (p. 69). Really by the sequence, the Euphratean analogy is merely stated. The stars of Scorpio cannot present at once the figure of a tree and a mummy (p. 69). A number of people have correctly identified both for me when I only indicated to them the general section of the sky in which to look. These identifications are tenuous, the rest beyond discussion (p. 70). I cannot sincerely express regret that my opinion differs as to this; but neither opinion proves anything. There is no detailed criticism of the other Peruvian sequences.

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[1] See Markham, *The Incas of Peru*, pp. 12–14, 263, 275, 278.

Passing to the Mexican and Maya sequences, Dr. Spinden objects to the division of the ecliptic among four independent series of asterisms comprised of ten, twelve, eighteen, and twenty members (p. 58). The so-called series of ten does not exist. This is explained below and also in my paper, but not by Dr. Spinden. The existence of three other series would be extraordinary. A study of Duran's drawings, which seemed to furnish evidence of asterisms pertaining to the months, has convinced me that no such series existed. Duran figures the eighteen months in connection with the monthly festivals and the details of these festivals show the influence of the zodiacal sequence. Undoubtedly they were governed by the zodiacal asterisms, probably both of the normal and day signs series. But they cannot represent an independent series of asterisms because only eleven asterisms are actually represented in a series of eighteen months and an intercalary period. These asterisms are scattered irregularly among the months and it is plain that the greater number of them, if not all, pertain to the day signs series. If each month were correlated with a particular asterism surely Duran would not have omitted so many asterisms. But if the months were merely governed by certain day signs used in sequence but without correlation in time we can understand why he might not consider it necessary to reproduce them all. This reduces the Maya and Mexican zodiacal asterisms to two series, the normal and the day signs. Two independent series of zodiacal asterisms are also found in India and Arabia. I owe thanks to Dr. Spinden for indirectly suggesting this correction.

The royal asterisms of Tezozomoc and Sahagun are described as one half of a ten part zodiac derived from native books and random information from Spanish writers of the second crop (p. 58). Here Dr. Spinden might not improperly have explained that one of the five signs mentioned is interpreted as the double sign Libra-Scorpio. The figure of the scorpion which covers both these signs is so conspicuous and unique in the sky that probably the desire to retain it as a whole led the ancient Maya and Mexicans as well as the ancient Greeks to figure it in some of their zodiacs as a single constellation covering two signs. The figure is badly distorted in our curtailed constellation. The Maya and Mexicans seem to have extended its opposite constellation in similar manner, perhaps for symmetry, thus forming a zodiac of eight single and two double asterisms. These corresponded with twelve signs presenting merely a variant form of the normal zodiac. The five royal asterisms therefore covered six signs or half of the zodiac, exactly the number of zodiacal asterisms that would be visible to the Mexican king when he went out under the night sky to observe them. This may seem somewhat more plausible than Dr. Spinden's statement of the case and to whatever crop of authors Sahagun pertains we undoubtedly owe to him more valuable information relating to ancient Mexico than is afforded by any other writer.

Numerous explanations of the Mamalhuaztli asterism are given which demonstrate the uncertainty regarding its position in the sky (pp. 58, 59). None of them are supported by a sequence or correspond with other symbols having a like position in other sequences.

Tianquiztli, the Market asterism is identified with the Pleiades by Tezozomoc (p. 59). It is the only instance among the five asterisms in which the sequential interpretation compels me to oppose either Tezozomoc or Sahagun. The view that the former is mistaken in this instance is supported by the impossibility of reconciling the outline of the asterism given by Sahagun with the Pleiades and by Tezozomoc's marked uncertainty about the other asterisms except the unmistakable Scorpion. The Mexican summary supports the sequential identification with Virgo.

The five asterisms could not have formed a continuous series along the ecliptic because Tezozomoc states that the first four marked the four directions (p. 61). On the contrary all the Mexican zodiacal signs seem to have been associated with certain cardinal directions, perhaps in more than one system. One assignment was based upon a natural comparison of the daily and annual course of the sun, rising in the east at the vernal dawn of the year, attaining his farthest north at the summer solstice, receding into the west at the autumnal equinox, and departing into the south during the winter. The equinoxial signs were accordingly placed east and west and the solsticial signs north and south and each of these four major signs carried with it its neighbor on each side, thus dividing the zodiac into four triplicities each assigned to a cardinal point. The association of the Mexican and Maya years with different cardinal points is well known. Perhaps Tezozomoc mentioned the four sequential signs not as referring each to a different cardinal point but as part of the zodiac which governed all four (p. 58). We may leave the royal asterisms with a denial that Sahagun places Mamalhuaztli in Taurus (p. 61). In Book 7, Cap. 3, he identifies the Mastalejos, the same asterism, with stars "near the Pleiades, which is (are in) the sign of the Bull."[1] According to his statement they may or may not be in Taurus.

The sequence of the Spear Throwers is objected to because it is associated with certain calendrical calculations that apparently pertain to revolutions of the planet Venus (p. 61). But whether sideral or synodical I can see nothing in them inconsistent with a basic zodiacal significance supplemented by Venus periods. The influences of Venus mentioned on page 62 are exerted from the zodiacal asterisms of the day signs against their opposite asterisms whose symbolism is correctly stated in sequence thus paralleling the scheme of the Spear Throwers in the day signs sequence.

The impressive correspondence between the sequential symbolism of the day signs and that of other zodiacal series is called a monument to ingenuity (p. 62) and no reference is made either in the text or bibliography to the additional correspondence between the zodiacal positions of the day signs as identified by me and their relations with the rainy and dry seasons as figured in the Codex Vaticanus 3773.[1]

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[1] Los mastelejos del cielo, que audan cerca de las cabrillas que es el signo del toro, Lib. 7, Cap. 3.

On the Aztec Calendar Stone (p. 62) I place Cipactli of the circle of day signs at the summer solstice because it is found at the top of the circle above the central solar head corresponding with the northernmost or topmost position of the sun in its annual course. The positions of all the other signs are then determined by the sequence. This is my only venture and I consider that its correctness is strongly confirmed. It is explained in my paper but not by Dr. Spinden. The day signs are unequally distributed among the signs probably because their asterisms were of unequal size. Evidence of their association with zodiacal asterisms is furnished by the correspondence between their sequence and other zodiacal sequences, their association with zodiacal sequences in codices, inscriptions, constellation bands, and ritual, and with the rainy and dry seasons in the Codex Vaticanus 3773. The Maya day sign Manik offers additional and impressive evidence on this point in the correspondence between its meaning, the Grasping Hand, and the configuration of the tail of the Scorpion, to which asterism it must be assigned in the sequence.[2] Seler regards the nine lords of the night of the Mexican codices as probably stars or constellations.[3] Each lord is accompanied by a day sign in correct sequence and the whole represents one less than half the asterisms of the vigesimal zodiac, practically all that are visible at a given moment, and they cover exactly the same half of the zodiac as the royal asterisms of Sahagun and Tezozomoc. Why ten lords were not represented is unknown to me.[4] The argument that the discrepancy between my zodiacal identifications of the day signs and those of other students of the subject proves the error of all (p. 65) is somewhat amusing, for it is the very argument that theologians have used and still use to disprove the truth of evolution. In every new field of study divergent views are almost inevitable as to details and they continue to exist long after the main principles have been established.

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[1] *International Congress of Americanists*, XVIII *Session*, pp. 137–140.

[2] Hagar, *International Congress of Americanists*, XVII *Session*, Mexico, 1910, pp. 140–159; *American Anthropologist* (N.S.), vol. 17, pp. 488–491.

[3] *Codex Fejervary-Mayer*, Loubat ed., p. 48.

[4] Seler also associated the twenty day signs with twenty stars but later found possible difficulties with this point of view which I fail to understand (See *Codex Fejervary-Mayer*, p. 171). Mrs. Nuttall asserts that they constitute a native zodiac but makes no attempt to identify any day sign with any part of that zodiac. (See *Fundamental Principles*, p. 255.) Humboldt and Beyer attempt to identify the

The next objection is that, as the leap year error was not interpolated in the Mexican or Central American year counts, although calculated very accurately, the months thus fell behind the seasons at the rate of about twenty-four days in a century. Hence the months could not possibly maintain a close time relation with the signs of the zodiac (pp. 65, 66). Granted, but need we assume that they did? I have explained above that the months possessed no zodiacal asterisms of their own, but were probably governed both by the twelve normal signs and the twenty asterisms of the day signs, also that these asterisms governed the months in sequence but as in our own zodiac, were not correlated with them in time. The principal use of the zodiac was divinatory rather than calendrical. However, the symbolic sequence was probably established at some date when it did synchronize with the months, but the subsequent divergence was disregarded as it is by us. Facts indicate that the only direct calendrical relations of either normal or day signs zodiac were with the annual festivals and that the dates of these festivals were determined by actual observation of the stars as they still are among some of the tribes of the United States.

But the Mexican and Maya months do not coincide in time as given in my sequences (p. 67). Probably because they were correlated with the zodiac at different dates. The sequence is similar in both and that is the critical element in these studies. Their present calendrical relations are not important in this field.

Scorpion figures in the Tro-Cortesianus codex do not represent the Scorpion asterism because they are associated with pictures

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asterisms of some of the day signs with different results from mine and Granados seems to assert their zodiacal basis without attempting identification (pp. 63, 64).

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of hunting (p. 68). Can they be pictures of scorpions actually hunting deer? The identification of the Scorpion with Scorpio; of Scorpio with the month Tzec, the Scorpion; of the grasping hand inserted in place of the scorpion's sting with the day-sign, Manik or Grasping Hand, of the tail of the Scorpion asterism, are all described as the wildest of guesses in spite of their proper positions in the sequence.

However, I insist upon continuing my incorrigible career by adding that the Maya word *chikin*, sting sun, name of the west, refers to the sun in Scorpio in accord with the zodiacal scheme of the cardinal points explained above, hence its glyph is composed of the manik sign which represents the sting of the Scorpion, and the kin or sun sign.

Contrary to the statement on page 68 my paper on Teotihuacan is based upon personal study of the archaeological remains at that place. The genuine antiquity of the maps upon which that paper is based is vouched for by Professor Marshall H. Saville and they give a fairly accurate plan of the city.

I am unable to admit that the account of Lizana and the survey do not justify the deductions in my Izamal paper. This is again a matter of opinion, but in that paper I expressly disclaimed that all the deductions were proven.

On page 71 Doctor Spinden mentions other zodiacs unknown to me that have been "imagined" for other American Indian tribes. The late Major J. W. Powell also imagined that the festivals of various tribes of American Indians "are fixed at times indicated by the signs of the zodiac." [1]

Now as to intercommunication between the different zodiacs in America and between them and those of the eastern continent. The principal basis of the argument in favor of intercommunication is the same as for the existence of the zodiac, namely, the complex and artificial nature of the various zodiacal sequences combined with the marked analogies between all of them. No one doubts that wooden dwellings are erected by primitive peoples on different continents without intercommunication, because everywhere man seeks protection from the rain and wood is usually the most abundant and easily obtained material for this purpose. Undoubtedly, many analogies in customs, languages, institutions, and rites are similarly explained as like results of like causes acting independently upon the same medium—the mind of man; but not all analogies can be thus explained. When one examines the members of the zodiacal series and realizes their artificial nature, the difficulty of explaining any one of them in the same manner for all times and places, and yet the similarity not of one or two, but of the entire series in sequence, to the writer's mind, in spite of admitted difficulties, it seems less miraculous to postulate intercommunication than to suppose that such analogies can be created by chance or by the law of like causes and like results. Probably there will be less opposition to the theory of intercommunication between the American zodiacs because of closer contiguity, racial identity, and the absence of an intervening ocean. Even Doctor Spinden asserts (p. 57) that agriculture spread from a single source over a large part of North and South America and carried with it certain ceremonies. Now the beginnings of the zodiac would probably synchronize closely with the beginnings of agriculture since it is closely associated with solar influence upon crops and this whether it is of native or foreign origin in America. Tribes of Mexican blood are now known to have been in contact with others of South American affinities near the Isthmus of Panama and Balboa heard of the Inca dominion at that point. The Maya contact with the Mexicans is an historic fact and as to the Pueblo ritual Mexican affinity or contact has been at least suspected by good authority.

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[1] Cushing, F. H., *Introduction to Zuñi Folk Tales*, p. xiv.

The same principles apply to the question of intercommunication with the eastern continent but here, of course, the difficulties are much greater, even when confined to the accidental and sporadic communication, probably in ancient times, which alone seems possible.[1] Why did the foreigners bring only the zodiac, without food products, artifacts, or any material objects, why no religious, political, or other ideas? We must answer that as to material objects shipwrecked sailors would have none, while as to ideas other than the zodiac it is by no means certain that they have not been received from the other continent; the probability lies the other way. But early navigators would be sure to emphasize the study of the stars and of the zodiac (p. 56). It is unfortunate that almost all the advocates of intercommunication have supported such ridiculous theories as naturally to prejudice the present scientific mind against the possibility of a rational defence of that view in any field, but, from the writer's viewpoint, some day the pendulum will swing back to the normal middle and this attitude will also be recognized as unscientific.

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[1] Hagar, *American Anthropologist* (N.S.), vol. 14, pp. 43–48.

Dr. Spinden states the following objections to intercommunication: Scandinavian settlements in Greenland and communication with Labrador have left no traces upon the Northern Indians (p. 56). Granting this still uncertain statement, the northern tribes were not as receptive as the civilized peoples of South and Central America.

Analogous forms result from a unifying tendency acting upon differing sources, or convergent evolution as Dr. Spinden terms it, rather than from the opposite process or divergent evolution, therefore they did not originate from a single source. Uninitiates found phantastic theories on the former while savants confine their attention to the latter (pp. 56–57). But convergent evolution could not produce the complex zodiacal sequences.

Therefore I remain convinced of the existence of the zodiac described by me in various parts of America and of the strong probability that its correspondence with Oriental zodiacs is due to intercommunication at some time and place as yet unknown, or, possibly, at several times and places.[1]

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[1] Two other of my zodiacal papers not previously mentioned by Dr. Spinden or by me will be found in the *American Anthropologist* (N.S.), vol. 13, pp. 229–234; vol. 14, pp. 525–529.

JUAN B. AMBROSETTI

By C. W. MEAD

DR. Juan B. Ambrosetti, director of the Eth Museum of the Faculty of Philosophy and Buenos Aires, Argentina, died May 28, 1917.

Dr. Ambrosetti stood in the foremost rank among gists, and his death at the early age of fifty-two year loss to science. From youth his bent was strongly toward science and art, and although a man of very considerable wealth he was, throughout his life, an indefatigable worker.

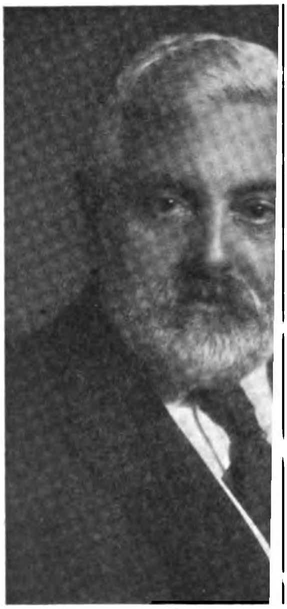

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Fig. 66.—Juan B. Am

At the time Dr. Ambrosetti took charge of the Ethnographical Museum its entire material consisted of a small collection of bronzes, donated by Dr. Indalecio Gomez. Today the collections reach 20,000 catalogue numbers. Quite a large part of this material he collected himself in the field.

Dr. Ambrosetti's most valuable contributions to archaeology are his works on the Calchaqui region of his own country. These give the field work during his various expeditions. These ex always financed himself. His writings have a natur style, and in his descriptions of the woods, the desol plains, and of waterfalls we feel his love of nature and

Dr. Ambrosetti always took a prominent part in the meetings of the International Congress of Americanists and will be much missed at future meetings. He was a man who made friends wherever he went, and has left many in this country, in different branches of science, who admired him as a man, and respected him for his scientific attainments. It has been said of him that "in his life of affection and teaching he strove to make friends of his disciples and disciples of his friends."

He was an official delegate of the Argentine government to the Second Pan American Congress at Washington, D. C., in December, 1915–January, 1916, where he was also the accredited delegate of the faculty of philosophy and literature, and the faculty of agronomy and veterinary medicine of the National University of Buenos Aires; Museum of the University of La Plata; University of Cordoba; Museum of Natural History of Buenos Aires; Board of American History and Numismatics; Argentine Scientific Society; and the Argentine Geographical Institute. He was president of the first session of the congress.

During the last year or two Dr. Ambrosetti had been preparing plans for a new museum building which should be in the true Tiahuanaco style. The collections of the Ethnographical Museum are housed in the basement of the building occupied by the Faculty of Philosophy and Letters, where the space is entirely inadequate for their display, and consequently a large part of the material is in storage. As he did not live to do this work it is to be hoped that the faculty will carry out his idea and give his name to the museum as a fitting tribute to one of the greatest scientific men his country has produced.

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[1] Nos. 1–60 were compiled by Dr. Ambrosetti and published in Buenos Aires in 1904. Nos. 61–76 are from a private letter from Dr. Robert Lehmann-Nitsche.

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    Mereció los honores de ser transcripta en "La Tribuna," números 996 y sig. y en los "Anales de la Sociedad Rural Argentina," tomo XXIX, No. 12.
11. Segundo viaje á Misiones por el Alto Paraná é Iguazú. Boletín del Instituto Geográfico Argentino. Tomo XV, 1894, 156 páginas con láminas y un plano de las Cataratas del Iguazú.
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Las treinta y tres notas de esta primera serie tratan de las siguientes materias: I. Ídolos funerarios; II. Ídolo de significación Incásica; III. Amuletos ó exvotos para el buen parto; IV. Amuletos para el amor (Huacanqui ó Cayan carumi); V. Ídolos femeninos de piedra? La Pacha Mama?; VI. Ídolo Tanga-tanga: Trinidad india; VII. Vasos votivos antropomorfos; VIII. Representaciones de Tigres; IX. Vasos ornitomorfos; X. Illas ó amuletos para los animales; XI. Figuras zoomorfas; XII. Divinidad Catequil (?); XIII. Morteros zoomorfos de piedra; XIV. El Peinado y el Tocado. Apéndices. XV. Ídolos falicos de piedra; XVI. Cetros de mando; XVII. Placas pectorales y Discos de bronce; XVIII. Los Incas no dominaron la región Calchaquí; XIX. Campanas ó tantanes de bronce; XX. Un bronce que no es Calchaquí; XXI. Ídolos músicos; XXII. Thoquis ó insignias de mando hechas de piedra; XXIII. Amuleto ofidiofálico para la lluvia; XXIV. E.

Símbolo del Suri; XXV. Muyunas ó torteros de huso; x
animales; XXVII. Un curioso sombrero Calchaquí; XXVIII.
funerarios y objetos antropomorfos; XXIX. Fumaron en
chaquíes?; XXX. Cuchara de madera; XXXI. El Símbolo del
Una nueva forma de peinado; XXXIII. Un producto de fundici

Y además contiene las siguientes invocaciones en quichua m
sembrar. Para hilar. Para señalar cabras. Para busca
Para andar en el cerro. Para cazar vicuñas. Para carne
no se pierda el ganado. Para enterrar un muerto. Para be

44. Rastros etnográficos comunes en Calchaquí y México. Anales
Científica Argentina. Tomo LI, 12 ps., 1901.

45. Noticias sobre la alfarería prehistórica de Santiago del Estero.
Sociedad Científica Argentina. Tomo LI, 15 ps. con grabad

(Se describe la alfarería fina de color rojo pintada de ne
ante con rojo, azul y blanco, y también una gran urna fune
con un símbolo antropozoomorfo.)

46. Hachas votivas de piedra (Pillan Toki) y datos sobre rastros d
Araucana prehistórica en la Argentina. Anales del Muse
Buenos Aires. Tomo VII, 15 ps. con una lámina y un graba

(Se describe un hacha con grafitos de la Pampa Centr
Nacional.)

47. Un nuevo Pillan Toki. Revista del Museo de La Plata. Ton
una lámina, 1902.

(Se describe otra hacha de piedra con grafitos de Choele-Ch
de La Plata.)

48. Algunos vasos ceremoniales de la región Calchaquí. Anal
Nacional de Buenos Aires. Tomo VII, 9 pgs. con grabados,

(Se describen: un vaso con borde recortado en terrazas, ot
interno y otro con impresiones de basketería.)

49. El sepulcro de La Paya. Recientemente descubierto en l
chaquíes. Anales del Museo Nacional de Buenos Aires.
30 ps. con grabados, 1902.

(Descripción del material arqueológico extraído de ese
perteneció á un jefe: adornos de oro, un hacha de bron
de madera, varios tipos de alfarerías con simbolismo neuvo y
Al final se hace un estudio iconográfico comparativo de l
mentos simbólicos con los ya conocidos y se señala la i
alfarería con la hallada en el Norte de Chile.)

50. Datos arqueológicos sobre la Provincia de Jujuy (antigüedade
Anales de la Sociedad Científica Argentina. Tomos LIII y
con grabados, 1902.

(Descripción de material arqueológico procedente en su r
Casabindo y Santa Catalina, con datos sobre mates pirogra
de ofrendas, alfarerías, objetos de piedra, madera, bron

sobre el Folk-Lore de los actuales habitantes, entre esto: el baile de los Chunchos.)

51. I Calchaquí. Conferencia dada en Roma en la Sociedad Geográfica Italiana, en misión especial del Ministerio de Instrucción Pública y publicada en el Boletín de dicha Sociedad, Cnaderno I, año 1903, 18 pgs. con grabados.

52. Las grandes hachas ceremoniales de Patagonia (Probablemente Pillan Tokis). Anales del Museo Nacional de Buenos Aires. Tomo IX, II ps. con grabados, 1903. (Se describen por la primera vez esas grandes piezas de forma aproximada de un manubrio plano, presentando una serie de todas las halladas hasta la fecha en el Chubut.)

53. Antigüedad del Nueva Mundo (Crítica al Dr. Latouche-Treville á propósito de un artículo aparecido bajo el mismo título en la Revue des Revues). Revista de Dericho, Historia y Letras. 16 pgs., 1903.

54. Los pucos pintados de rojo sobre blanco del Valle de Yocavil. Anales del Museo Nacional de Buenos Aires. Tomo IX, 13 ps. con grabados, 1903.

(Se describen 18 objetos de este tipo, los únicos hallados hasta hoy, y el desarrollo de su simbolismo. El material pertenece á los Museos Nacional y de La Plata.)

55. Cuatro Pictografías de la región Calchaquí. Anales de la Sociedad Científica Argentina. Tomo LVI, 13 ps. con grabados, 1903.

(Se describen, pictografías de las Quebradas de las Conchas, Bodega y Chuzudo (Quilmes), Provincias de Salta y Tucumán.)

56. Cabeza humana preparada según el procedimiento de los Indios Jivaros, del Ecuador. Anales del Museo Nacional de Buenos Aires. Tomo IX, 5 ps. con una lámina, 1903.

(Descripción de un ejemplar único, donado al Museo Nacional por el Dr. Estanislao S. Zeballos. Se trata de una cabeza de un blanco, reducida.)

57. Insignia lítica de mando de tipo chileno. Anales del Museo Nacional de Buenos Aires. Tomo XI, 8 pgs. con grabados, 1904.

(Descripción de una especie de maza de piedra del Sur de Mendoza.)

58. Informe del Delegado de la Universidad de Buenos Aires al Congreso de Americanistas, XIII Sesión de Nueva York.

Revista de la Universidad de Buenos Aires, tomo I, 42 páginas, 1904.

59. Apuntes sobre la Arqueología de la Puna de Atacama. Revista del Museo de La Plata. Tomo XII, 30 ps. con 4 láminas, 1904.

(Descripción del material del Museo de La Plata, con un estudio especial sobre los escarificadores de madera, y datos sobre Pictografías y Petroglyfos como el de Antofagasta, Peñas Blancas, Infieles, etc.)

60. El bronce en la región Calchaquí. Anales del Museo Nacional de Buenos Aires. Tomo XI, 149 páginas con grabados, 1904.

Datos sobre antiguas minas y procedimientos metalúrgicos de los Calchaquíes y descripción de todo el material conocido hasta hoy que se halla en los Museos y colecciones del país y del extranjero. Con un estudio sobre el simbolismo de las placas pectorales y discos.

61. Exploraciones arqueológicas en la Pampa Grande (Provincia de Salta).

Publicaciones de la Seccion Antropologica de la Facultad de Filosofia y Letras de Buenos Aires. No. 1. 200 páginas, Buenos Aires, 1906.

62. El hacha de Huaycama. Anales del Museo Nacional de Buenos Aires. Tomo xvi pág. 15–23, 1906.
63. Exploraciones arqueologicas en la ciudad prehistorica de "La Paya" (Valle Calchaque, provincia de Salta), campañas de 1906 y 1907. Publicaciones de la Seccion Antropologica de la Facultad de Filosofia y Letras de Buenos Aires. No. 3, 534 páginas. Buenos Aires, 1908.
64. La bolsa de una média prehistórica ? de Vinchina (provincia de la Rioja) (Nota arqueologica). Anales del Museo Nacional de Buenos Aires. Tomo xvii. p. 215–223. 1908.
65. Clava litica de tipo peruano del territorio del Neuquen. Anales del Museo Nacional de Buenos Aires. Tomo xvii p. 229–231. 1908.
66. La Facultad de Filosofia y Letras de la Universidad, Nacional de Buenos Aires y los estudios de arqueología americana. Anthropos Tomo iii, p. 983–987. 1908.
67. La question Calchaquie et les travaux de la Faculté de philosophie et lettres de l'Unuversité Buénos Aires. Verhandlungen des xvi Internationalen Amerikanisten Kongresses. Wien 1908.
68. Congreso internacional de Americanistas Viena (1908)—xvi sesion. Memoria del delegado de la Universidad Nacional de Buenos Aires. Revista de la Universidad de Buenos Aires. Tomo xi, p. 87–115. Buenos Aires 1909.
69. Un objeto raro de alfareria de Misiones. Apuntes de historia natural, I, p. 124–126. Buenos Aires 1909.
70. Un documento gráfico de etnografia peruana de la época colonial. Publicaciones de la Seccion Antropologica de la Facultad de Filosofia y Letras de Buenos Aires. No. 8. 27 páginas. Buenos Aires 1910.
71. Idolo zoomorfo del Alto Paraná. Contribucion á la etnología americana. Anales del Museo Nacional de Buenos Aires. Tomo xxi, p. 385–393. 1911.
72. Nuevos restos del hombre fosil argentino (Presentacion de dos cráneos del hombre de Guerrero, Provincia de Buenos Aires). International Congress of Americanists Proceedings of the xviii Session, London, 1912, p. 5–8.
73. Memoris del Museo Etnográfico 1906 á 1912. Publicaciones de la Seccion Antropologica de la Facultad de Filosofia y Letras de Buenos Aires. No. 10, 47 páginas. Buenos Aires, 1912.
74. Doctor Florentino Ameghino, 1854–1911. Anales del Museo Nacional de Buenos Aires. Tomo xxii, p. xi–xv, 1912. Reproducido en Anales de la Academia de Filosofia y Letras. Tomo ii, p. 203–262, 1914.
75. El Museo Etnográfico de la Facultad de Filosofia y Letras como auxiliar de los estudios de ornamentación aplicables al arte en general. Revista de arquitectura I No. I. Buenos Aires, julio de 1915.
76. Prof. Pedro Isalabrini (1849–1916). Anales del Museo Nacional de Buenos Aires. Tomo xxviii, p. 227–239, 1916.

BOOK REVIEWS

METHODS AND PRINCIPLES

An Introduction to Social Psychology. **Charles A. Ellwood.** Appleton and Co.: New York, 1917.

Social psychology or psycho-sociology is for Dr. Ellwood the foundation not only of sociology, but of all the social sciences at large. Its prominence is based on the psychological nature of its interpretations (p. 8). It is a central theoretical thesis of Ellwood that all social life is psychic in the sense that it presupposes the mental interaction of the individuals that make up the social group. He repudiates the idea of a super-individual group-soul and makes the significant statement that the group cannot be understood apart from its individuals, nor these apart from the group (p. 19). The psychological terms in which social life is to be interpreted he designates as those of "inter-stimulation and response" (p. 324). It is the interaction of the individual psyches that brings about social evolution.

It is important for the understanding of Ellwood's line of thought that his idea of the psychological nature of social life makes certain definite implications. While he alleges to assume a purely pragmatic attitude towards the subject matter of psychology by including in it the states of consciousness as well as the neural processes (p. 8–9), he at once implies a factor which commits his whole point of view to that of evolutionary psychology. He takes it for granted that "modern psychology bases itself upon modern biology" (p. 20) and without the least attempt to examine this dogma critically he proceeds to transfer the biological principle of adaptation and of natural selection into his psychology. From this position alone is Ellwood enabled to deduct his psychological generalizations and to evaluate social processes in a scheme of higher and lower stages. Indeed, without the idea of psycho-social evolution this type of social psychology would be an absurdity.

In this way the question of the relation of social to organic evolution gains especial importance for Ellwood's argument. In the second chapter of his book he is quite explicit on this question. Social life is a superstructure of the organic. By the natural process of the survival of the fittest the organic has developed always higher forms of adaptation until it merged by the working of the selfsame principle in the cycle of social evolution. The superorganic characteristics of social control are simply "fitter" life forms. Their origin goes far back into the history of animal life. Cultural evolution is a further projection of the scheme, it is the human phase of social evolution (p. 46).

By regarding the social as superorganic, but by applying to it at the same time the principles of organic life contradictions become unavoidable. Some of Ellwood's inconsistencies on this point are interesting. Thus on page 180 he writes:

> Social evolution . . . is not comparable either in nature or method to organic evolution.

But elsewhere (p. 145) he designates as one of the forms of unconscious social change the process of natural selection. And again on page 32 when speaking of intelligence he says that purposeful activity, like all mental life, is developed by natural selection. As a matter of fact the direction of Ellwood's argument, as it progresses, is determined at every point by his scheme of psycho-biological evolution. It is reflected no less clearly in his purely psychological conceptualizations. There are different "levels of behavior": The lower ones are those of instinct and of habit, the higher ones those of cognition, of thought and feeling (p. 58). These are the elements of the psyche of the individual, but they are also direct correlates of the stages of evolution. Cognition, which finds its highest expression in the human intellect, is the crowning point of the evolutionistic pyramid. It

> stands revealed as the ultimate and final factor in social adaptation (p. 214).

And wherever Ellwood comes to speak about the human intellect he bursts forth in eulogy of this highest instrument of social control. How could it be otherwise when the evolutionistic dogma is taken for granted? From this point of view civilization is conceived in its typical aspects as the expression of the intellectual interaction of individuals.

> Civilization . . . is decidedly an intellectual achievement (p. 214).

While Ellwood theoretically repudiates an intellectualistic interpretation of history, he constantly applies it himself. For instance he states that

> language must be regarded as chiefly an intellectual creation (p. 69),

and again:

> It is, indeed, the modification of man's original tendencies through the intellect which has built up civilization (p. 203).

While Ellwood's evolutionism determines the whole range of his developmental reconstructions, it at once breaks down when he formulates his norms for the future progress of social life. Social progress for him aims towards a state of perfected social control, it is

> increasing adaptation to the requirements of social existence which shall harmonize all factors . . . in the life of humanity, securing the greatest capacity for social survival, . . . and the greatest possible harmony among all its varied elements (p. 291).

Now there can be no doubt that this harmonious millennium would represent an absolute self-negation of the process of natural selection, a neutralization of the forces that developed the social from the biological, the intellectual from the instinctive. A consistent doctrine of social evolutionism would demand a state of affairs in which natural selection could work most freely and most effectively.

It is at this point, I think, that Ellwood's intellectualism gets the better of his evolutionism. If I understand him correctly, his assumption is that the intellect is not merely relatively, but absolutely the highest possible form of adaptation and that, therefore, natural selection is eliminated as soon as it holds sway. Thus, in a state of perfect social control there will be no place for a selection of the fittest. Such a view of Ellwood's position seems to be justified by such statements, for instance, as that the standards of science

> must be accepted by all rational minds, because they rest upon established facts (p. 283).

The intellectualism of this standpoint is absolute.

The anthropologist is naturally interested in Ellwood's methodological principle of utilizing anthropological data in the interpretation of social phenomena. He is, however, less appreciative when he sees these forced into an evolutionistic and intellectualistic scheme, when Ellwood, for instance, insists throughout his book on characterizing the stages of primitive man as those of "savagery" and "barbarism." Ellwood's criterion for primitive culture is its low degree of rationalization which for him is synonymous with socialization (see for instance p. 64). It is precisely the data of anthropology that teach us the irrelevancy of such evaluations and when studied critically they show how "primitive" in many respects the socialization of our own life is when compared with certain types of primitive culture in which the social values permeate almost the entire complex of the people's life (compare for instance certain forms of totemism). To insist on the higher form of our state of socialization is simply to insist arbitrarily on certain features in the complex of phenomena and to disregard others. The same obtains when social life is viewed from the aspect of adaptation to surroundings.

When, for instance, the "control over nature" is taken to be symptomatic of the absolute height of our culture, then the emphasis is laid simply on one of the many possible types of adaptation. And, to be sure, a high degree of one type of adaptation, say of an economic one, is only too often directly associated with the decline of another type of adaptation, probably with one of moral or aesthetic standards. Cultural life may show the most heterogeneous types of adjustment. A primitive social group may possess an infinitely higher intensity of adjustment to life from the point of view of religion and mythology than our own. Or are spiritual values not a part of life-forms? What of it if primitive shamanism is not "rational" in *our* sense of the word? Do we satisfy our spiritual values by rational formulae? It is precisely the preponderance of one comparatively narrow type of adaptation—a type of which the sociologists make so much—that threatens to leave our life as a whole almost devoid of cultural values.

As to the effect of our rationality on the future course of social life I regard it as preposterous to expect from science "the harmonious adjustments" of all of its elements, the progress "from a mythological and fictitious to a positive and scientific stage" (p. 213). What problems can science solve that will determine now and forever the adjustments of our life-values? To misunderstand in this way the dynamic complexity of our adjustments seems to me to be the fallacy of the evolutionary psychologist. He interprets cultural life in terms of his own metaphysics.

Ellwood has remained true throughout his book to the principle which he has formulated at the very outset, namely that social life "in order to be understood scientifically, must . . . be related to the general theory of social evolution and organization."

> Just as the theory of organic evolution is the necessary foundation for any right development of the biological sciences, so the theory of social evolution . . . is the necessary foundation for the proper development of social sciences (p. 2).

In accordance with this principle Ellwood's psychological concepts are almost all deduced directly from his "general theory" and are necessarily of the most general type. Instinct, habit, intelligence, which correspond to different "stages," are evolutionistic derivatives. It is hard to see how "psychological" understanding can be gained by using such terms for interpretations, by conceiving, for instance, military institutions and government as having developed out of the "fighting instinct" and "private property" out of the "instinct of greed" (pp. 199–200) or by explaining cultural borrowing by "imitation" (p. 240). Certainly, generalized abstractions of this kind cannot enhance our psychological

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understanding of the phenomena. In order to understand a process like cultural borrowing psychologically we must strive to comprehend as intensively as possible the concrete conditions of the individual cases. This intimate understanding of social phenomena. I think, is really what we mean when we insist so frequently on the psychological aspect in anthropology. There is nothing like an abstract psychological understanding of cultural phenomena.

HERMAN K. HAEBERLIN

AFRICA

Harvard African Studies I: Varia Africana I. Editor, ORIC BATES, M.A., F.R.G.S.; Assistant Editor, F. H. STERNS, Ph.D. The African Department of the Peabody Museum of Harvard University: Cambridge, Mass., 1917. 9 + 292 pp., numerous pls. and text figs.

According to the editorial announcement this magnificently printed and illustrated publication is the first of a series of similar volumes to be issued annually in the interest of African studies. Purely historical papers are to be barred except in so far as they shed light on anthropological questions and Egyptian articles will deal exclusively with the prehistoric period and the primitive aspects of Nilotic civilization. On the other hand, objective investigations on the somatology, sociology, and folklore of the American Negro will be welcomed. For the entire program the coöperation of both American and European scholars is invited—indeed, several of the papers here bear witness to the intercontinental character of the enterprise.

The breadth of view that has guided the editors is apparent from the titles of the articles in the present volume: M. M. 'Abd Allah, "Sîwan Customs" (9 figs.); E. A. Hooton, "Oral Surgery in Egypt during the Old Empire" (2 pls.); J. Roscoe, "Worship of the Dead as practised by some African Tribes;" F. H. Sterns, "The Paleoliths of the Eastern Desert" (map and 18 pls. with 130 figs.); H. D. Mathews, "Notes on the Nungu Tribe, Nassawara Province" (2 figs., 1 pl.); J. Abercromby, "A Study of the Ancient Speech of the Canary Islands;" E. A. Hooton, "Benin Antiquities in the Peabody Museum" (7 pls.); A. Werner, "The Utendi of Mwana Kupona;" R. H. Blanchard, "Notes on Egyptian Saints" (3 pls.); G. A. Reisner, "An Inscription from Gibel Barkal" (1 pl.); O. Bates, "Ancient Egyptian Fishing" (5 text figs., 26 pls. with 254 figs.); R. F. Carroll, "Selected Bibliography of Africana," "Editorial Notes" (8 text figs., 1 pl.).

The most cursory glance at the volume shows that archaeology, ethnography, and linguistics are all represented without undue favoritism and the very full footnotes and bibliographies accompanying some of the papers indicate the scholarliness of the authors. What seems to the reviewer of even greater importance is the fact that in the longer essays the emphasis is not on purely descriptive detail, considerable attention being devoted to the matter of distribution and to theoretical points. Thus, in his Benin article Dr. Hooton calls attention to the influence of Frobenius' Ifé finds on the Benin problem (p. 130), and Dr. Sterns in his brief description of some "Darfûr Gourds" connects their ornamentation with that of Hausa gourds. Kabyle and Upper Egyptian pottery, West Sudanese carving, and northern Africa generally (p. 195). This is, of course, exactly as it should be. Comparative summaries are always in place and give direction to the fieldworker's inquiries.

The longest of the papers is Mr. Oric Bates's "Ancient Egyptian Fishing." Since the reviewer must disclaim even the slightest pretensions to Egyptological learning, he can only record his general impressions. In spite of several digressions the article makes interesting reading and certainly conveys a wealth of concrete information, whether all the suggestions voiced by the writer be accepted by Egyptologists or not. Quite rightly, he does not confine himself to the technological side of the fisherman's activities, but also takes in some of their sociological aspects.

Of the other articles it is simply possible here to refer to Dr. Sterns' detailed description of "The Paleoliths of the Eastern Desert" of Egypt. With exemplary caution the author discusses the possible correlations with European chronology. Though the implements resemble most closely those of the Mousterian period, he insists that all conjectures as to age must remain purely hypothetical until stratified sites yielding comparable material or fossil remains in satisfactory association with such flints have come to light (p. 78).

From a technical point of view an index and legends to accompany illustrations would be very desirable.

All ethnologists must joyfully welcome this addition to the literature of their science. Given the editorial broadmindedness in point of subject-matter and method of treatment, the series cannot fail to aid materially in the solution of African problems, of which there are many and tantalizing ones.

Robert H. Lowie

NORTH AMERICA

Tsimshian Mythology. FRANZ BOAS. Thirty-first Annual Report of the Bureau of American Ethnology. . . . Pp. 29 to 1037; 3 pls., 24 text figs. Washington, 1916.

Under the too restrictive title of *Tsimshian Mythology* Dr. Boas has recently issued one of his most remarkable ethnographic studies of a West Coast people, and a vitally important contribution to the theory of mythology and social organization.

His voluminous memoir (1008 pages) has, through many accessions, grown around a nucleus of oral traditions (334 pages[1]) recorded in the course of twelve years, at Port Simpson, B. C., by Henry W. Tate, an educated half-breed. The bulk of the monograph itself consists of "Description of the Tsimshian, based on their mythology" (pp. 393–477), "Tsimshian society—social organization" (pp. 478–564), "Comparative study of Tsimshian mythology" (pp. 565–881), and a few short sections containing a sketchy description of the Tsimshian (pp. 43–57), a summary of comparisons of mythological plots and themes (pp. 936–958), a list of Tsimshian proper names and place names (pp. 959–966), a glossary, and an index of references (pp. 967-1037).

In the Appendix, we find the extraneous "Myths of the Bella-Bella, collected by Livingston Farrand," and "Myths of the Nootka, collected by George Hunt" (pp. 883–935).

The lengthy section on comparative mythology is the one that first attracts our attention here. In thoroughness nothing surpasses Dr. Boas' comparative study of various mythological plots and themes in the traditions of the Tsimshian and other British Columbia peoples—parallels seldom being sought outside.[2] The analysis of the Raven and Transformer cycle, for instance, and the survey of its diffusion in the Northwest, alone covering 156 pages, may well be considered as exhaustive and final, in so far as published material goes; and no fewer than 45 versions recorded in British Columbia are utilized in the study of the "Test theme." The systematic demarcation and comparative study of cycles and plots are here accomplished even more extensively than in the works of Paris, Cosquin, Bolte and Polívka folk-tales. Although no departure from accredited meth involved, it is noticeable that students of American myth whole, have heretofore classified and listed parallels of themes rather than of plots and cycles—or complexes of incidents.[1]

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[1] About 85 pages of narratives from the same source appeared separately, in 1912, under the title of "Tsimshian Texts (new series), by Franz Boas," in the *Publications of the American Ethnological Society*, vol. III. These had been preceded by "Tsimshian Texts, Nass River dialect, recorded and translated by Franz Boas," in Bulletin 37, *Publications of the Bureau of American Ethnology* (1902).

[2] Dr. Boas has here resumed in a more specialized way his earlier study, embodied in his *Indianische Sagen* (1895).

A conclusion reached by Dr. Boas is that, notwiths interrelations, a cleavage is apparent between the myth Northern and Southern nations of the Coast, and that plateaus, although connected with the other areas by such the Skeena river, are likely to form a third distinct group.

We may quote here a still more important parallel b American and European folk-lore (p. 877—):

> The distribution of plots and incidents of North American fo a strong contrast when compared to that found in Europe. tales, while differing in diction and local coloring, exhibit remark of contents. Incidents, plots, and arrangement are very muc wide territory. The incidents of American lore are hardly les buted; but the make-up of the stories exhibits much wider di sponding to the greater diversification of cultural types. . . . lore creates the impression that the . . . stories are units, cohesion is strong (and) the whole complex very old. The analy material, on the other hand, demonstrates that complex storie there is little cohesion between the component elements, and tha parts of tales are the incidents and a few simple plots.[2]

The following minor conclusions regarding the form thology in British Columbia are no less interesting, as th to other areas as well (p. 874—):

> . . . All these examples illustrate that there are a number plots, which have a wide distribution, and which are elaborated incidents that are literary devices peculiar to each area. . . .
>
> . . . We find also certain incidents that have a very wide occur in a variety of plots. . . .
>
> . . . The processes of invention and diffusion of plots mu from a point of view entirely different from that to be applied invention and diffusion of incidents. The latter are, on the modifications of every-day experiences, and not likely to develo

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[1] The "Summary of Comparisons" (pp. 936–958) will greatl sultation and reference.

[2] A fairly large number of themes and incidents, in Europe as w seem to be older than the complexes embodying them, as their inde is wider and locally more intensive.

> with a frequency sufficient to explain their numerous occurrences over a large area. On the other hand, stories of . . . contests between two villages, of a rejected lover, are so closely related to every-day experiences, and conform to them so strictly that the conditions for the rise of such a framework of literary composition are readily given.
>
> . . . The present investigation shows also that the imagination of the Indians revels in the development of certain definite themes, that are determined by the character of the hero, or that lend themselves in other ways to variation. . . .
>
> . . . We find the same incidents in various connections, and this makes it clear that it would be quite arbitrary to assume that the incident developed as part of one story and was transferred to another one. We must infer that the elements were independent and have been combined in various ways. . . .
>
> . . . In a number of cases . . . a psychological connection of the elements of the complex story is sought.

In concluding, this section broadens into a masterly general theory of mythology, with especial reference to the now moribund "naturist" or "a-prioristic" schools of mythologists, in whose belief the mythologies sprang independently in different parts of the world from the uniform and essential activities of the human mind coupled with the contemplation of nature and, more particularly, of the sky phenomena. Let us quote again (pp. 879–881):

> . . . I insist that the attempt to interpret mythology as a direct reflex of the contemplation of nature is not sustained by the facts.
>
> . . . We are justified in the opinion that the power of imagination of man is rather limited, that people much rather operate with the old stock of imaginative happenings than invent new ones.
>
> . . . It seems reasonable . . . to base our opinions on the origin of mythology on a study of the growth of mythology as it occurs under our own eyes. . . . We have no reason to believe that the myth-forming processes of the last ten thousand years have differed materially from modern myth-making processes . . . .
>
> . . . The contents of folk-tales and myths are largely the same. . . . Neither group can claim priority. . . . If we once recognize that mythology has no claim to priority over novelistic folk-lore, then there is no reason why we should not be satisfied to explain the origin of these tales as due to the play of imagination with the events of human life. . . . Practically all the supernatural occurrences of mythology may be interpreted by these exaggerations of imagination. . . . The facts . . . do not show that the elements of which these tales are composed have any immediate connection with the phenomena of nature, for most of them retain the imaginative character just described. . . . The material presented . . . shows nothing that would necessitate the assumption that it originated from the contemplation of natural phenomena.

> . . . I am not prepared to admit that the present condition of myths [with reference to the anthropomorphization of the sun and moon, of mountains and animals] indicates that these form any important part in mythology.
>
> That European myths happen to have developed in this direction . . . does not prove that we must look for a poetic interpretation of nature as the primary background of all mythologies.

Although restricted in scope, the "Description of the Tsimshian, based on their mythology" and the "Tsimshian Society" deserve our attention, as an important question of method is here raised.

Dr. Boas has utilized the oral traditions[1] of the Tsimshian in his possession as a basis for a description of their social organization, customs, arts, and material culture.

> Material of this kind, he says, does not represent a systematic description of the ethnology of the people, but it has the merit of bringing out these points which are of interest to the people themselves. They present in a way an autobiography of the tribe.

We agree that Dr. Boas's patient classification of the native traditional accounts incidentally illustrating the customs and mode of life is highly useful and gives a fairly accurate idea of the ethnography of the Tsimshian. Yet the reader should not forget that information of that nature can constitute only secondary evidence. We have just seen that myths and tales are usually the common patrimony of tribes scattered over vast and not necessarily homogeneous culture areas. As they are only to a slight extent adapted or transformed to suit the milieu, it is fair to suppose that such variations alone are illustrative of local peculiarities. The bulk being of a cosmopolitan nature, an ethnographic description based upon it should prove broadly representative of a more or less defined type of culture rather than of specific differentiations. A superficial reader of some English or French folk-tales, for instance, is often apt to believe that they accurately represent the moral and intellectual traits of local peasantry, while a scholar may establish that they have been imported in their entirety from Asia or elsewhere. In the same way a Tsimshian traditional account of the deeds of a powerful shaman or the like may in the first place have been a reflex of an adventure of a Kwakiutl or Nootka sorcerer.

A description of a local custom or belief based upon such data is likely not to be adequate or, in some cases, correct.[2]

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[1] In a few cases the author has also drawn from other sources of information.

[2] The same remark might possibly apply to such studies of old-world history as have been based upon similar folk-lore materials, *Life in the Homeric Age*, by Seymour, for instance.

Although Dr. Boas's description of the Tsimshian is convincing and probably accurate on the whole, we have found it imperfect in the only field that we have tested—that of social organization—owing to the fact that the text material furnished to him by Tate was one-sided and very incomplete.

We must not, however, rush to the other extreme, as we are not at all inclined to share in the lofty scepticism of a well-known American ethnologist as to the usefulness of the Indian lore for historical purposes.

Valuable critical insight might be gained from a careful classification of the various types of oral narrative, some of which are bound to reflect more concretely than others a definite milieu, especially when they are given as accounts of actual events.

The Tsimshian, according to Dr. Boas, distinguish two types of stories: the myths (*ada'ox*) and the tales (*malEsk*).[1] More categories, however, are clearly discernible. The cosmogonic, aetiological and hero myths, and the folk-tales—although not on a par to the natives—are much of the same nature for historical purposes; they drift from tribe to tribe without becoming individualistic in their form and contents. Myths of origin of a clan, a crest, or the power of a chief, on the other hand, are more pregnant with local traits and mentality, notwithstanding their conventional and traditional plots. Accounts of a war, a battle or a migration are still more closely dependent upon a real occurrence and its effects upon the faculties of the witnesses that first handed them down. Vainglory, exaggeration and distortion at their worst cannot, here, entirely veil the reality. And an ethnographic sketch based on a large mass of many-sided narratives bearing on the history of the tribe would no doubt be realistic.

Dr. Boas's statement,

> The collection here presented evidently contains the bulk of the important traditions of the Tsimshian

leaves us somewhat sceptical. Four hundred pages of oral traditions in all seem little for a West Coast nation formerly consisting of at least fifteen populous tribes (Tsimshian proper), scattered over a wide territory and possessing a well developed phratry and clan system. Recently acquired experience in the same field, moreover, has impressed us with the magnitude of the task for one who would attempt, even at this late day, to collect all the traditions referring restrictively to the separate origin of the clans and families, and to their inherited privileges. Almost endless are the stories that one can collect on the wars, adventures, and troubles that still constitute the complex background of the Tsimshian mentality.

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[1] P. 565.

While Dr. Boas's texts represent well the types of transcendent tribal myths, tales, and traditions, they fall short of the requirements when they are expected to yield a satisfactory perspective of a confused domestic history and an intricate social structure. Our opinion, on this point, is based upon an intensive field analysis of the Tsimshian social organization, supplemented by a large number of myths and relations—very few of which appear in *Tsimshian Mythology*—collected by the reviewer and his assistant Beynon, at Port Simpson and *G̯itgx̣a'ła* (1915–16).[1]

Why did Tate collect general myths and tales rather than local or special ones? The reasons for this are fairly clear. The narratives of the first type are the property of all; any informant at large may know and repeat them. Quite on the contrary, the second belong restrictively to a clan, a house or a chief. Not even the breakdown of the old order of things has yet abolished the deeply seated jealousy of the natives as to what formerly was their exclusive privilege. No native, especially in the presence of another, will relate the tradition that concerns another; it would be, to say the least, a breach of etiquette. We have noticed, moreover, that these are little known, except by hearsay, to outsiders. Tate, who shared in his compatriots' corrosive diffidence, does not seem to have overcome these barriers. He is not likely to have consulted many outside of his own family members. Hardly any of our twenty-five representative informants had been utilized by him. The fact that he himself belonged to the lower class (a Raven clan in the *G̯itzaxłɛł* tribe, if we remember well) may not have made him *persona grata* with most of the chiefs—royal or others.

The foundation of Dr. Boas's conclusions as to the origin of the *G̯ispuwudwa'də*, the Wolf, the Raven and the Eagle phratries seems to illustrate our point as to how misleading is secondary and incomplete evidence, when too much reliance is placed upon it. Let us restrict the discussion here to two instances, those of the myths of *G̣ao''a'* (no. 49,

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[1] The following numbers, in Dr. Boas's series, seem to refer more especially to the history and privileges of outstanding social units or to tribes as a whole: 12, 39, 41, 44, 45, 46, 47, 48, 49, 50, 51, 52, 64, and, in the supplement: 1, 2, and 3. The demarcation between historic-like traditions or myths belonging exclusively to clans and families and those that form part of the general stock is not clearly drawn here. For instance, the exclusive myth of the Wolf Clan (no. 52) is placed next in the series to "The Prince and Prince Wolf," an unattached tale ending with an aetiological myth.

G̣auō!) and of *'Niəslarano·ᵒs* (no. 64, Story of the Wolf Clan). The first describes adventures of four mythic ancestors whose crests correspond to those of a few modern *G̯ispuwudwa·'də* families, and whose home was *Ṫəmlax'a'm*, a village located on the upper Skeena River plateau. A first step in Dr. Boas's induction is indicated in these statements (p. 523):

> . . . According to tradition, part of the *G·ispuwudwᴇ'da* are apparently the only division of the tribe that constituted the ancient Tsimshian. . . .
>
> Prairie Town (*Tlᴇm-lax-am*), the original home of the Tsimshian (p. 394). . . .
>
> It might almost seem as though, in the opinion of the Indians, the tribe had consisted originally of this group only, and that other groups had developed by accretion (p. 486). . . .
>
> It seems probable that at an earlier time the Tsimshian lived on the upper course of the Skeena river. According to their own belief, they lived then in the village *Tlᴇm-lax-am* (p. 483). . . .
>
> Many years before I knew that the Tsimshian held any such belief, I had expressed the conclusion that the Tsimshian must have been an inland tribe (p. 525).

Dr. Boas further thinks it possible that the four exogamic groups were already represented at *Ṫəmlax'a'm* (p. 524). He finally concludes:

> . . . The Tsimshian take a somewhat exceptional position among neighboring tribes, and seem to be recent intruders on the coast (p. 872).

This thesis, however, is not entirely borne out by the facts at our disposal. That many *G̯ispuwudwa·'də* families claim the *G̣ao''a'* myth as their own and as explaining their origin offers no doubt. Who are these families? (1) The *Ṫəmlax'a'm* royal clan, branches of which are to be found in six Tsimshian tribes; (2) the *Ṫəmlax'a'm* lower clan, belonging to two tribes; and (3) the *G̯itksədzɔ'* clan, with three ramifications. Not all of these families, however, claim the myth in the same way; and only three or four royal households are considered as the actual descendants of the four mythic sons of *G̣ao''a'*, and, as such, enjoy the use of their crests. But these families constitute only the minority in the *G̯ispuwudwa·'də* phratry, which may contain as many as nine clans with different myths, crests and privileges. Many of the latter, termed *Lax'mɔ'n* (sea-coast) clans, convey the firm impression of being very ancient. The *Təmlax'a'm* clan, it is considered by the natives, are fairly recent intruders among the Tsimshian, being an offshoot of the interior Gitksan nation; and although the *G̯itksədzɔ'* clan claims the same remote ancestors, their members are more immediately a branch of the *Ṇisgɛ* (Nass river) *G̯ispuwụdwa·'də*.

Deductions based upon the myth of *G̣ao''aʻ*, therefore, would apply only to the 33 households of the three above-mentioned clans out of an approximate total of 79 households in the *G̯ispuwudwa·'də* phratry.[1] In other words, 46 *G̯ispuwudwa·'də* units have either originated from other neighboring nations or have always been Tsimshian, in so far as traditions and other evidence go. According to our census, there were about 147 households included in three other phratries (or exogamic groups). None of these accepted the myth of *G̣ao''aʻ* as their own. To conclude, while about 33 Tsimshian social units admit having Gitksan ancestors, this is denied of nearly 193 others. The proportion of 33 to 193, therefore, represents well the size of the interior elements that grafted themselves upon the already existing Tsimshian nation. What was the ultimate origin of the Gitksan *G̯ispuwudwa·'də* (termed *G̯israhɛ·'s*)? As the *G̣ao''aʻ* myth purports that crests were introduced among a people who had none, one may wonder as to whether it is not a reflex of an extension towards the interior of the crest system, the center of diffusion of which is evidently the Northwest Coast. If this hypothesis is worth consideration, its logical corollary is: had not the descendants of *G̣ao''aʻ* West Coast ancestors?

A few words will here suffice in connection with "The story of the Wolf clan" (No. 64) and the resulting conclusions of Dr. Boas, crystallized in the following sentence (p. 486):

> The Wolves are said to have come from Stikine River, and they are considered as descendants of a group of Tahltan who fled from their country and settled partly on the coast of Alaska, partly on Nass River, and partly in Skeena River. Their story is told on p. 354.

The brief and incomplete narrative referred to is considered by native informants as describing the history of *'Niəslarano·'ᵊs*, of the Wolf phratry, and royal chief of the *G̯itlɛ'n* tribe. That *Niəslarano·'ᵊs'* ancestors are likely to have been Tahltan is generally admitted. But if we consult our Tsimshian list of houses, we find that this theory applies only to one out of thirty *Laxḳibu* (Wolf) households, among the Port Simpson tribes, all of whom have different traditions, names and crests. While this royal house is admittedly a recent accession in the Tsimshian system, many Wolf clans reasonably entertain a belief in their own local antiquity, namely those of *'Asaral'yɛ·'n* (in the *G̯itsi·'ᵊs*, *G̯ispa'x'lɔ·ts* and *G̯inax'ang̯i·'ᵊk* tribes) and of the *G̯inaʻdɔ·'ⁱks* tribe. Hardly any phratry is so well represented within one tribe as is the Wolf phratry in the *G̮inaʻdɔ·'ʻks.* Three ancient clans and their ramifications are here represented with a list of at least fourteen households. Even if we were to discard the testimony of the natives regarding their antiquity, internal evidence would tell the same tale, as time alone can produce so large an expansion of undisturbed kinship groups.

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[1] We refer here to the tribes still represented at Port Simpson.

Similar remarks apply, to a corresponding extent, to Dr. Boas' theories of origin of the Eagle and the Raven phratries (pp. 486 and 524).

A brief reference may also be made here to Dr. Boas' following theory (pp. 483, 485, 487):

> It would seem, on the basis of the data given here, either that the older form of social organization of the Athapascan, Tsimshian, Haida and Tlingit, and perhaps also Bellabella, was based on a threefold division, or that the first three tribes developed a third group, that took a somewhat exceptional position. . . .

That is, there would have been, at the basis of the system, three original phratries, each of which may now be found, in some places, subdivided into three parts (clans).[1]

In the light of our data, we fail to see how this theory could be true of the Tsimshian proper. Some elements in each of the four phratries have been part of the nation from a time immemorial; and although the older elements in the Eagle phratry are not so clearly defined as in the case of the other groups, they cannot be altogether dismissed.[2] It is not without some surprise that one finds this theory expressed in *Tsimshian Mythology.* A partisan of the "a-prioristic" school might well argue that the mystic number "three" spontaneously bursts forth in different parts of the world, as a result of some occult psychic necessity. But Dr. Boas, as we have seen, strongly and rightly combats such an antiquated notion. The number "four"—instead of "three"—moreover, is the current mystic number on the West Coast and in most of North America. There is no reason then for any strenuous effort being exerted in discarding facts that conflict with this theory; and Dr. Boas himself, we feel assured, does not believe in the essential, basic and persistent necessity for a tripartite system in the social organization of the Northwest Coast and Plateau nations.

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[1] In a personal communication received at the last moment Dr. Boas says: "I am not at all interested in the question of 'three,' but only in the point that it is not a two-fold division."

[2] The phratries themselves are subdivided respectively into approximately the following number of clans or units: the *G̮ispuwudwa'dɔ*, 11 or 12; the Raven, about the same number; the Wolf, 7; and the Eagle, more than 6.

Although more special in character, the section on "Tsimshian Society" (III, pp. 478–564) contains much material of general interest. More than any other it will attract the attention of students of social organization and of West Coast ethnology. It should be noted that a substantial part of the data here set forth are not derived, as in the other cases, from Tsimshian mythology, but from diverse sources, particularly from direct observations of which H. W. Tate made lengthy and valuable records in his prolonged correspondence with Dr. Boas.

While the less conspicuous chapters on birth, marriage, death, burial, war, potlatch, religion, secret societies and shamanism are not likely to occasion conflicts of opinion, those on "Social Organization" (divisions and clans, terms of relationship—comparative—, social rank, crests and names) and "Comparative Notes on Social Organization of the Tsimshian," on the contrary, are teeming with controversial matters; and we regret that space here forbids our engaging in a tempting discussion of many statements of fact as well as of theoretical problems. We will, therefore, restrict our remarks to a very few points.

We cannot help feeling that, had Dr. Boas had a prolonged opportunity of studying in the field Tsimshian village, kinship, and clan organizations, he would have revised many of his views on the subject, resulting as they do partly from the scantiness of his data and from the evident lack of insight evinced by Tate.

Let us first consider the nature of the tribal or village organization of the Tsimshian.

> The peculiarities of the Tsimshian system are due primarily to the small number of recognized villages and of distinct families and to the strict division of the whole people into a few tribal groups (p. 527).[1]

While it is not easy to disagree with this general statement, we cannot see the facts entirely in the same light. To us, the tribal organization of the Tsimshian and their neighbors is on the whole fundamentally similar, and differs only in degree of development and secondary matters. Were the Tsimshian tribes and families really so few, compared with those of the Tlingit and Haida? The tribes of the Tsimshian proper—the largest third of the whole Tsimshian people—were not long ago at least fourteen in number; each of these had distinct permanent and temporary villages and inherited fishing, fruit-gathering, and hunting territories, scattered far apart along the West Coast, or the Oxtall and Skeena rivers. This number, at the present day, is still maintained with modifications. The modern village of Port Simpson itself is geographically subdivided into nine out of the ten ancient component tribes. The list of clans, in the Port Simpson tribes only, recently included more than 30 units,[1] in the four phratries. These clans were further subdivided, so far as could be remembered, into approximately 116 families and 226 households; and there is a likelihood of many having escaped the memory of our informants. In this respect there does not seem to have existed any essential contrast between the Northwest Coast nations.

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[1] See also p. 482.

The next question is: What were the tribes or villages made of? Dr. Boas's answer is:

> The villages are generally described as belonging to a certain exogamic group. . . . It must be recognized that, even if in the early times the houses were the property of members of one exogamic group only, nevertheless a great many families of other groups must have lived in the same village (p. 529). . . .
>
> Although such a village was the property of a subdivision of one group [exogamic] necessarily a considerable number of individuals must have lived in the same village as husbands or wives, as the case may have been. It is probable that in this way the present conditions originated, the recent villages consisting of a number of house groups inhabited by different branches of the groups (p. 482). . . .
>
> The conditions may have been the same as among the Kwakiutl, where a continuous village site is divided into sections, each being the property of a subdivision of the tribe (p. 529).[2]

From the first to the last of these statements Dr. Boas proceeds in the right direction. None of the Tsimshian tribes and villages—possibly except at their very beginning—belonged exclusively to one or even two exogamic groups (phratries). In every one, save three, of the nine Port Simpson tribes, the four phratries were represented and were owners of property. The *G͜itzaxlɛ'ł*, the *G͜ina‘dɔ·'ⁱks* and the *G͜it'andɔ·'* tribes are the only exceptions, the Eagle phratry not being represented in the first two, and the Wolf phratry in the last. There usually was, within a tribe, a fair balance of power and wealth between two or three phratries; and we could give many instances of rivalry between opposing clans causing the disbanding of the tribe or, at least, internal dissension. Prompted by ambition or jealousy, a group of closely related families within a tribe was only too glad to advance its standing; failing other means, they readily welcomed into their midst a higher branch—possibly royal—of their own phratry or clan, who assumed their leadership. This explains the presence in many tribes of royal families of foreign extraction, and in a few cases of more than one royal family within one tribe. To illustrate the complex structure of one tribe, we will refer to that of the *G̯ispax'lɔ·'ts.* It consists of 5 *G̯ispuwudwa·'də* families (belonging to 4 clans and including 19 households), 2 Raven families (= 1 clan, 3 households), 8 Eagle families (= 5 c., 21 h.), and 1 Wolf family (= 1 c., 3 h.).

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[1] The smallest of these—mere remnants of larger ones or offshoots of foreign units—consisted of one or two families.

[2] See also p. 395.

At right angles with the tribal arrangement (or geographic unit) was the kinship organization. Not only the phratries, but also the clans, were to be found permanently represented in many tribes. If Dr. Boas here avoids using the terms of "phratry" and "clan,"[1] it is because their identity, at first sight, is not so clear as might be expected. It may take some time, even in the field, to pick up the tangled threads of the elusive kinship system, as the family units are the only groups the objectivity of which is plain. The clans consist of many closely related families, claiming the same myth of origin and the same leading crests and privileges, and possibly residing in different tribes. Their outlines remain concealed to the outsider until he is informed of the myth, the crests and derived functions, all of which have now fallen into disuse. The *Ṫəmlax'a'm* royal *G̯ispuwudwa·'də* clan has embraced no less than six royal families, in as many tribes. The *G̯itnagun'a'ks* clan (of the same phratry) is ramified into more than five branches, located in four tribes. The *G̯itsga'ⁱṛə* clan of the Raven phratry has five families in three tribes; and so on.

The question of rank is another one which we should like to discuss at greater length here. The rigid, permanent or hereditary distinction between families of royal (*səm'ɔ·'ⁱgət*) and of lower (*ləka'g̯ɛ't*) standing seems to be quite the same among the northernmost nations of the coast. We were surprised, however, to find out that the *Nisg̣ɛ'* (Tsimshian) differ from them in this respect. They are not, as is the case among the Tsimshian proper, headed by royal families of different lineage.[2] They consist merely of clans and families, each of which is under the leadership of its own directing kinsmen. In other words, they have no centralized tribal chieftaincy (or Haida-like "town mother").

The social advancement of boys, among the Tsimshian proper (see p. 498), can be obtained only within their own class. Although fluctuations in rank (p. 509) are possible, they are only gradual. Nothing short of a disaster can shift a family out of the royal caste; and the process of promoting one into it is scarcely more speedy than in the European hereditary nobility. Such is the deeply seated feeling attached to former acknowledged rank, in Port Simpson, that it is likely to survive that resulting from any other native institution. At the local government school, the little girls whose parents were of royal standing are still shown abject subservience by others, notwithstanding the teachers' opposition and the pervading modern conditions.[1]

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[1] See pp. 483, 488, and 500.

[2] *Nisg̣ɛ'* informants themselves acknowledge that this is one of the chief differences between themselves and their neighbors.

Dr. Boas' views on the crest system of the Tsimshian should not be overlooked. On page 527 we read:

> The head chief of each of the four groups [phratries] possessed all the prerogatives of the whole group and was its highest representative. Among the Haida his function did not extend beyond that part of the family represented in the village community. . . . Owing to the greater independence of the Haida families each has its own set of crests.

Here again the facts as we know them do not seem to justify such a contrast between the Haida and the Tsimshian. So exclusive and restrictive was the ownership of any valuable crest, among the Tsimshian, that not a single one really was the common property of a collectivity. Hardly any crest, except very low ones, had replicas and could be used by more than one person at a time, each being known singly under a special name. Although there were, for instance, a number of Raven crests, not two were strictly alike, and the characteristic differentiation of each was what made them the property of some one in particular. A crest without a myth to explain its origin and its connection with the owner was an impossibility; and such a myth was in the patrimony of a clan or a family. In a kinship group, we must add, the use of a crest was the privilege of one of the highest members. In that way the use of the many crests belonging to a clan soon became specialized. The virtual rule is: one crest, one owner. We cannot, therefore, agree with Dr. Boas' statement (p. 527):

> Owing to the small number of subgroups and the similarity of their crests, there are only a few crests that are not common property of the whole exogamic group.[2]

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[1] In the same recent communication, Dr. Boas adds: "The great difference between the Tsimshian and the other tribes evidently is due to the very fact . . . that the chiefs had much greater prerogatives than among the other tribes."

[2] Dr. Boas, in his communication, adds: "My whole line of argument is that the crests as held by the people are new and based on the older concepts of a few crests that are common to the whole division."

The lists of crests furnished by Tate (pp. 503–506) are of little value, as they are, in many cases, inaccurate, and never indicate their owner. Lack of space here forbids us to make more than a passing reference to Dr. Boas' interesting dissertations on the system of relationship (p. 490), exogamy and its historical development (pp. 518, 523, 528); crests, totemism and taboos (pp. 416, 502, 516, 529) and supernatural helpers (p. 513).[1]

We will now conclude with a brief review of the method adopted in recording the Tsimshian myths here presented.

In the preface, we learn that they were recorded for Dr. Boas, in the past twelve years, by W. H. Tate, a half-breed Tsimshian, of Port Simpson, B. C. Dr. Boas has himself written the myths in English from the literal interlinear translations accompanying Tate's Tsimshian texts. In the immediate preparation of these narratives Dr. Boas has shown great skill and patience. The material itself is of high quality both in form and content. And if Tate was not guided by scientific methods, his shortcomings were more than compensated by his life-long familiarity with the environment of his subject. Even an expert would experience great difficulty in penetrating so deeply into the sanctuary of aboriginal traditions and mentality. In such ability lies the undisputed usefulness of trained native informants or interpreters to the historians of their people. And Tate is only one of many deserving assistants in the study of the West Coast ethnography, among whom we may mention: George Hunt (Kwakiutl), Alex Thomas (Nootka), and William Beynon (Tsimshian).

In reading these Tsimshian texts, however, we should not entirely overlook the personality of Tate as a factor in their mode of expression. Dr. Boas is aware of the possibility of his having eliminated some traits that seem inappropriate to us; he adds that a few tales "bear evidence of the fact that" he "had read" published collections of Kwakiutl and *Nisgẹ'* traditions.[2]

While in Port Simpson, we have learned that Tate was not in the habit of taking down the stories under dictation. He was loth to divulge to other natives that he was really writing them down at all. Our assistant Beynon knew only of his "keeping a little book at home for those things." The fact that he had made such a large collection was practically unknown in Port Simpson.

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[1] The supernatural helpers indicated here are part of the secret society system.

[2] See pp. 31 and 721.

In writing down from memory a lengthy and complex tradition, Tate is liable to have forgotten or slightly altered many accessories or even supplied some out of his own stock of familiar notions. We have noticed, at one or two places, that his information as to the identity of crests spoken of in myths differs from that which we recorded with expert informants. Tate, moreover, relates these stories as if he were speaking to a stranger. For instance, he says (p. 389): ". . . In olden times, people cleared their land with stone axes. . . ." Such details on culture perspectives do not enter into the undisturbed Indian narratives. Interpolation of a more important nature is to be found in the myth of *G̣ao''aʿ*.[1] After having given the full text of the myth, which, in four other versions in our possession ends without explaining its connection with present-day social units, he goes on with more than four pages of explanation on the origin of the Tsimshian (and Tlingit) phratries, clans, crests, tribes, relationship and so forth. Interesting as may be a native's attitude towards the problems of ethnology, it is usually far from being a criterion of truth. It should, besides, reveal itself under its own colors. Tate's views here are not altogether acceptable and as they are supposedly part of a traditional text they are decidedly misleading.

The phonetic signs and transcriptions used by Dr. Boas in representing Tsimshian sounds and words show an astonishing grasp of the language, especially for one who has had little direct contact with the natives. In the first batch of Tsimshian texts (1912), written down by Tate and revised with the assistance of Archie Dundas, we detect in Dr. Boas' phonetic signs a strong reminiscence of his earlier personal knowledge of the *Nisg̣ɛ'* language. For instance, *q* in Nass river dialect is often the equivalent of *x* in Tsimshian: *hak!ulâ'q* (sea monster), in the first, becomes *hak'wəlɔ''x*, in the second; *Gul-qa'q* becomes *Gwəlg̣ɛ·'x*, and so on. Although these earlier *Nisg̣ɛ'* preconceptions are barely noticeable in *Tsimshian Mythology*, they occasionally reappear, as in the transcription of *g̣āᵒq* (raven) for *gɛ·x*, *g·ibā'yuk*, *łpō'n*, *lax-k·ebō'* and others. It goes without saying that the actual hypothetical reconstructions from Tate's most incomplete writing (based on the incorrect Ridley system) might well be revised at places, as in the case of *G·inadâ'ᵒx̣s* for *G̱inaʿdɔ·'ʿks*,[2] *G·its!ᴇmgā'lôn* for *G̱itsəmg̣ɛ·'ləm*, *Sagagwait* for *Sg̣agwe·'t*, *Nēs-awatk* for *'Niəs'awɛ·'tk*, *Asagulyaan* for *'Asag̣al'yɛ·'n*,

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[1] *Publications of the American Ethnological Society*, vol. III, pp. 215–225.

[2] Our remarks apply only to the real differences implied here, and not to the recent modifications in the phonetic signs as recommended by the Committee on Phonetics of the Bureau of Ethnology.

Astoē'nē for *G̣astu'i·'nə* and so on. It seems, moreover, that there might be an improvement in the phonetic equivalents of certain sounds. We would prefer seeing *'l*, *'m*, *'n*, *'y*, and *'w* instead of *l!*, *m!*, *n!*, *y!*, and *w!*, as these sounds are preceded, and not followed, by a glottal stop. We have noticed that the Tsimshian pronunciation of *x* is attenuated compared to that of the *Nisgɛ'*. Glottal closures are very frequent in Tsimshian, although in most cases Dr. Boas had no means of detecting their presence. In his phonetic key he even refrains from defining them or of ascribing a sign to them, except in the case of the consonantal unsimultaneous releases of the glottis. In his texts, however, we occasionally find his familiar ʻ sign. The ', which we now use for glottal stop, has a different function in his writing (inst. *sᴇm'ă'g·id*); we see it defined only in his earlier (1912) Tsimshian publication as, "a pause; when following an initial or terminal mute, it tends to increase the stress of the latter." Examples of words with glottal stops, as we have recorded them, follow: *tsəm'a'ks*, *mɛs'ɔ'l*, *hana''ᵃx*, *ła''ᵃx*, *Ṫəmlax'a·'m*, *G̯itnagun'a'ks*, *txa'ni·'*, *'nɛ·x* (fin), and *nɛ'ᵊx* (black fish). Many long-drawn consonants in Tsimshian should also be indicated: *e. g.*, *G·i-ludzā'r* might become *G̯il·odza·'ṛ*, and *halait*, *hal·ɛ·'it*. Another sound does not seem to have been described so far in Tsimshian; it is a deep uvular *r*, which we represent by *ṛ*. While *r* as defined by Dr. Boas applies fairly well to the *r* in *'Niəsyaranɛ·ᵊt*, *Leg̯israg̯ɔ·'*, *'Niəs'omarɛ·'*, *'Niəslarano·'s*, it fails to convey an accurate idea of one of the strangest sounds in Tsimshian—a deep uvular and almost untrilled *r* uttered while the tongue is flattened and raised in the middle towards the palate (the neighboring vowels being strongly influenced in the same direction)—and which we encounter in the following instances: *dzagadila·'ṛ*, *dəṛədɛ't* (or *də'ṛɛt*), *G̯il·odza·'ṛ*, *nə'ṛən*, *łəṛəm* and *G̯ĭtsəla·'səṛ*.

C. M. BARBEAU

PHILIPPINE ISLANDS

Traditions of the Tinguian: A Study in Philippine Folklore. FAY COOPER COLE. Field Museum of Natural History, Anthropological Series vol. XIV, no. 1, pp. 226. Chicago, 1915.

In this volume of myths and tales from one of the more important of the pagan tribes of northern Luzon, Dr. Cole has given to students one of the first really adequate collections of material on Philippine mythology and folklore. The ninety-one tales form one of the largest collections yet published from any Philippine tribe, and have the further distinction that the majority of them were obtained in text form, so that we have thus thoroughly reliable material, not reworked or retold for literary purposes.

The tales themselves are preceded by an excellent introduction in which the whole mass of data is classified and discussed. A division into three groups is made, (1) Tales of the mythical period, (2) Ritualistic and explanatory myths, and (3) Fables and folk-tales. In connection with the first group, an interesting attempt is made to reconstruct the mode of life and thought portrayed, and this is then compared with the existing modes. In a few particulars, such as the marriage of near relatives and the use of terrace agriculture, the mythic period differs from modern times, but in general the tales reflect a type of life and thought substantially like the present.

Some of the myths refer to localities lying considerably to the south of the present habitat of the tribe, and seem thus to point either to a northward migration of the people, or to a wider trade relationship in the past. It is pointed out that the religious ceremonial today shows a higher and more complex organization than among the neighboring interior tribes, and it is suggested that this may be due to the influence of contact with outside cultures. This conclusion is strengthened by a study of the tales themselves, as these show far more evidence of incorporation of non-Philippine elements than those so far accessible from the interior tribes. So far as the tales are concerned, this foreign factor is pretty clearly of Indian origin, but to what period it goes back, it is as yet difficult to say. Similar Hindu influences made themselves felt in Borneo, Java and Sumatra much more strongly, and there may be attributed, at least in part, to the invasion of Indian culture in the early portion of the Christian era. It seems probable that the extension of this influence to the northern Philippines was much later, and was brought about by trade or other contact with the partly Hinduized Javanese or Borneo tribes.

Of Chinese influence little appears in the tales, although evidence of Chinese contact, direct and indirect, is shown in the importance of magical Chinese jars and plates. The period of the earliest introduction of these wares into the Philippines can be placed with fair certainty somewhere about the tenth to the thirteenth centuries. The great value set on these products is still a characteristic feature of the present day life of the people, as it is also of several of the Borneo tribes. That as in so many cases, later cultural elements are incorporated in mythic tales of much earlier origin, is shown by the fact that tobacco culture is frequently referred to.

A comparison of the tales with those of the neighboring regions brings out many features of interest. As already stated, there are a considerable number of incidents and even whole tales, which are characteristic of the strongly Hinduized parts of southern Indonesia and the southeastern portions of the Asiatic mainland. The more typically Indian trickster fables are however, largely lacking, although those found in northeastern Indonesia and western Melanesia are well represented. Quite a number of tales belong to what I have elsewhere tentatively called the Indonesian stratum, and are characterized by their occurrence further east in Micronesia, Melanesia and even in Polynesia, and their absence, so far as known, in southeastern Asia and Java. On the whole, the general impression given is that of a primarily Indonesian substratum, upon which a later mass of Hindu-Malay elements has been spread. Had we only as full and reliable material available from a dozen other tribes in the Philippines, then a comparison of the data would go far toward unraveling some of the puzzles of cultural development in this most interesting and important region.

R. B. Dixon

PHYSICAL ANTHROPOLOGY

The Development of the Human Chin. W. D. Wallis. The Anatomical Record (1917), vol. xii, no. 2, pp. 315–328.

It is since the discovery of fossil remnants of palaeolithic man that the study of the mandible received a new impetus. Not only has the comparative morphological side of the problem been discussed extensively, but also the physiological factors to which adaptive changes are due, *viz.* use, non-use, speech, and such changes as come under the caption of convergence phenomena. The beginning of the specific investigations can be set in the year 1866, when E. Dupont discovered and described the "La Naulette" fragment. "For the first time," says Otto Schoetensack in his excellent work on the Heidelberg mandible discussing the La Naulette point, "was the scientific world disturbed by the observation of 'pithecoid' qualities in a human mandible." And early in the eighties of the preceding century, Gabriel de Mortillet commented on the speechlessness of that fossil man, emphasizing the fact of the wanting *Spina mentalis interna.* Several finds of primitive fossils have since brought forth a small deluge of specific literary products, and a number of theories on developmental points. Hermann Klaatsch, whose untimely death we deplore, himself an observer of extraordinary ability and connected with the salvage of many a costly find of later

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date, must be mentioned as showing new perspectives in the study of the mandible. By establishing his alveolar horizon he invented a practical, useful, and heretofore wanting method for the comparative study of an essential part of the cranium.[1] We are indebted to him also for a careful revision of the nomenclature concerning the mandible, with the probable exception of his "negative chin." By this is meant the recession of the chin behind a perpendicular erected on the alveolar horizon in the "incision," the measuring point between the two middle incisors on the alveolar ridge of the lower jaw, in analogy to the "prosthion" of the upper jaw. As "negative" would at the same time and certainly on first consideration imply the lack of a chin, which holds only for the Heidelberg specimen and the Anthropomorphae,[2] the accuracy of the term is somewhat questionable. Phylogenetic changes in the teeth in connection with functional changes have also found an open field for ample theorizing. Careful investigations in the palaeontological field have considerably broadened the aspect, and facilitated and approved argumentation of a decisive character.

In a sort of critical review based on the principal works written on the subject, Wallis deals especially with the chin question. A few historical remarks are followed by sections on speech; decrease in the size of teeth and of the alveolar processes; other osseous changes; muscular and mechanical forces; and changes in head form. We are told that the ancient Greeks when in the act of supplication touched the chin to show their divine affinity and that Pliny recognized the human chin in contradistinction to other animal forms. Linnaeus was the first to observe the chin and that Cuvier attempted the first comparative study of the mandible and offered an explanation for the evolution of the chin by advancing the theory that the retraction of the alveolar process came about in order to fit the originally smaller upper jaw. Thus the chin was produced, a theory substantiated much later by M. Pelletier,[3] when she showed the independence of growth between the alveolar part and the corpus of the mandible. The influence of speech, on the other hand, upon the formation (1) of the *Spina mentalis interna* and, (2) upon the chin itself, by the increased application of the *Mm. genioglossi*, *geniohyoidei* and *digastrici*, is an unsettled question as yet. It is true though, that the spinae are distinctly anthropine formations, when grooves are found in the apes for the insertion of the first two muscles as also in some human fossils where at least the *M. genioglossus* shows such conditions (Le Moustier, Mauer, Krapina G.). It seems to me that the physiological meaning of the spinae should not be attached solely to the function of speech. Rudolph Martin reminds us that the muscles concerned are used in the art of mastication, too, exactly as they are used by lower animals. It may appear to the reasoning mind then, that feeding as a fundamental requirement for the preservation of life and indeed much more strenuously necessitated than speech might be considered as the factor that produced grooves of insertion in the animal and spinae in the man. That the agency of speech cannot simply be eliminated is true and might have helped develop and hand on a new product. The causes underlying this human peculiarity must surely be traced to other phenomena and Wallis is perfectly right when he says (pp. 225–226):

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[1] Klaatsch, Hermann, "Kraniomorphologie und Kraniotrigonometrie," *Archiv für Anthropologie*, N. F. vol. VIII, no. 1/2, pp. 101–123.

[2] But a very interesting statement is made by Emil Selenka in the second chapter, page 143, of his "Studien über Entwickelung und Schädelbau, *Schädel des Gorilla und Schimpanse*," to this effect: "Auffallend ist die Bildung eines *deutlichen Kinnes bei einigen kindlichen Gorillaschädeln* mit entwickeltem Milchgebiss."

[3] "Contribution à l'étude de la phylogénèse du maxillaire inférieur," *Bulletins et Mémoires de la Société l'Anthropologie de Paris* (1902), ser. 5, v. 3, p. 537–545.

> The various portions of the skull are so closely interrelated, either structurally or functionally, that any considerable change in a given region is apt to be reflected by corresponding or compensating changes throughout the entire skull.

And this leads us back to the human chin, its preparatory changes and final establishment. Changes "throughout the entire skull" are preëminently due to changes of the brain, the development and formation of its hemispheres and the consequent expansion of the brain case. It may be recalled here that Bolk has very plausibly demonstrated that the so-called migration of the *Foramen magnum* is also caused by cerebral expansion towards the occipital, showing that its position was rather stable.[1] Compensatory changes in the lower jaw would then be the broadening out of its condyles and subsequent adaptations in the angles, branches, body, and anterior plate. If we add the changes brought about by the decrease and adjustment of the teeth, the retraction of the alveolar processes both in the upper and lower jaws, the necessity of broadening their forward portions, muscular adjustments, we have enough evidence on hand for the explanation of the morphological peculiarity of the chin. Walkhoff's "trajectories" and Mies-Toldt's *Ossicula mentalia* would be found necessary then for meeting the strain of transformatory exertion in the anterior plate. It does not seem entirely impossible to me that by a gradual cross-tension of the anterior plate in several directions the insertion grooves on its inner surface might have been leveled out by and by; the never ceasing muscular action, increased if one likes by efforts produced in speech, causing the new insertional tuberosities to form spinae of different size as we find them in man.[1] The retraction of the alveolar process going on steadily helping in forming the *Impressio subincisivia anterior* (Klaatsch) the formation of the chin is on its route. Klaatsch says directly, on page 11 of his paper named above, that the *Protuberantia mentalis* must not be considered a newly acquired but as a negative magnitude, conditioned by the persistence of this anterior part of the mandible on the level and in the rounding which this region originally possessed. It is this rounding which Wallis also recognized in the simiae, explaining it as the result of the flattening action of the platysma. In his chapter on "Muscular and Mechanical Forces" a good many pointers are launched that help to shed light on this interesting morphogenetic question. Quite logically the author finally concludes that no one factor should be singled out for having evolved the chin. He has read widely on his subject and given a comprehensive resumé. However a few inadequacies must not be allowed to pass unmentioned. We are agreed on the fragment of "La Naulette," not simply "Naulette" (p. 317). But as we find it correct on the next page, a mere *lapsus pennae* may have occurred. But *lapsus pennae* are surely not accountable for "the Mauer and Krapina G *skulls*" (p. 317); the frequent use of "simia" as a plural form; "the small hyoid bone" (p. 318) instead of the small horn of the hyoid bone; the "os mentale" (pp. 321 and 324) in place of for instance "regio mentalis"; the misspelling of G. Ruge as "Rugge" (pp. 322 and 323); the statement that "the lower jaw is not anatomically a part of the skull" (p. 325); and the other statement, that "widening of the dental arcade gives more play for the tongue, whether for shifting food or *consonants*" (328).

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[1] Bolk, Louis, "Über Lagerung, Verschiebung und Neigung des Foramen magnum am Schädel der Primaten," *Zeitschrift für Morphologie und Anthropologie*, 1915, v. XVII, pp. 611–692.

BRUNO OETTEKING

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[1] Walkhoff has observed very correctly that: "Vergleichend ist leicht zu erkennen, dass bei den tiefstehenden Rassen die Kieferformen noch weit voller sind, während bei den zivilisierten Nationen die Leistenbildungen samt den Knochenvorsprüngen weit mehr zur Geltung kommen." He also speaks of a certain economy in formative material exercised by nature in the higher forms. See Otto Walkhoff, *Der Unterkiefer des Anthropomorphen und des Menschen in seiner funktionellen Entwickelung und Gestalt*, 1902, pp. 226 and 227.

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DISCUSSION AND CORRESPONDENCE

THE MATRILINEATE AGAIN

IN a recent memoir of the association which publishes this journal, Mr. E. Sidney Hartland has reopened affirmatively the question of the priority of matrilineal kinship. He reviews, in a manner most amiable and polite to those who have expressed themselves negatively, the evidence from North America and Australia, the two greatest areas of exogamy and totemism; and pleads "for a reconsideration, on the part especially of North American anthropologists, of the North American facts."

The essential facts, and the essential considerations which have led the majority of ethnologists in the North American field to the interpretation that the view of the priority of matrilineal reckoning is without foundation in their field, are contained in the basic paper on "The Social Organization of American Tribes" published in this journal by Dr. John R. Swanton a dozen years ago. Brief as is its compass, it seems that there is little of intrinsic moment bearing on the issue, either in the way of evidence or the use of evidence, that is not contained or implied in Dr. Swanton's paper. It should have essentially disposed of the matter for a generation at least. But now that a scholar of the learning, acumen, and restraint of Mr. Hartland has reëntered the deserted arena with a challenge, it is only fair to conclude that he may represent also the views of others. I shall therefore attempt to restate and amplify Dr. Swanton's interpretation with a directness which his characteristic modesty would probably forbid him.

Excluding Mexico, upon which the evidence remains to be systematized by a Mexican specialist, native North America contains three areas in which definite exogamy with definitely unilateral descent prevailed. The first and largest of these comprises the territory east of the hundredth meridian and south of the fiftieth parallel, or a little beyond. The exact boundaries of the area of course are not straight lines. The western limit, for instance, protrudes farther on the north than in the south. For most of the tribes on the immediate Atlantic coast from the Carolinas to New England, the evidence is defective. So far as it goes, however, it indicates them to have been exogamic and unilaterally reckoning.

The second area comprises the Southwest, that is, the tribes of New Mexico and Arizona; also, according to recent information secured by Mr. E. W. Gifford, which I may cite because it is at this moment in press, the Indians of the greater part of the southern half of the adjacent state of California.

The third area consists of what it is customary for ethnologists to call the Northwest Coast or the North Pacific Coast of America, from the Tlingit in southern Alaska to the Kwakiutl in southern British Columbia, with an adhering fringe of tribes in the hinterland.

Now, five bodies of facts emerge.

1. Outside of the three regions mentioned, there occur no specifically exogamic groups, and descent is either bilateral, that is, indeterminate, or attached only to inheritance of property and station and not to inheritance of an affiliation determinative or regulative as regards marriage. There may remain a few tribes in the vast territories excluded from the three exogamic areas, which on fuller knowledge may prove to belong with the exogamic peoples in the three areas. They are certainly not many, since the North American tribes have been examined so extensively that the result is almost that of a systematic inquiry, and the subject of marriage and descent has so long been in the forefront of attention that the probability of revolutionizing data having been overlooked is exceedingly small. Practically, therefore, the existence of a great irregular but continuous territory, covering about half of the continent, and inhabited wholly by non-exogamic peoples, must be admitted.

2. Within each of the three exogamic areas, substantially every tribe —so far as present knowledge goes, absolutely every tribe—adhered to a scheme of social groupings that were concerned with marriage and were inherited in one line of descent only.

3. In each of these three exogamic areas, both matrilinear and patrilinear descent was found among different tribes. In the East, the Muskhogeans and Iroquoians formed the bulk of a compact body of matrilineal nations, partly surrounded by a continuous belt of patrilinear tribes, mainly Algonkian and Siouan. A second, smaller, and detached matrilineal group consisted of the Mandan, Hidatsa, and Crow on the western edge of the area. In the Southwest, the Pueblos and Athabascans were matrilineal, the Piman, Yuman, and Californian tribes patrilineal. The Northwest was matrilineal except for the Kwakiutl. The institutions of the Kwakiutl are certainly abnormal; but if the primacy of their father right be questioned, it should be

remembered that all the tribes to their south unquestionably transmitted family crests, rank and title, badges of honor, and wealth, from father to son, even though lacking intrinsically exogamic divisions.

4. In each area the most advanced tribes, those which, so far as their history is known or may without unreasonable speculation be inferred, have produced a type of civilization, are matrilineal. Those less advanced, which have absorbed or simplified their civilization rather than created or added to it, are patrilineal. Signs of such advancement are agriculture; town life; an intricate economic system; a development of industrial processes or the arts, as statistically determinable by the number of kinds of manufactures; an elaborated religion with numerous and interrelated ceremonies; the ability to develop coherent political institutions. It is true that backward tribes are sometimes matrilineal. The matrilineal hinterland Athabascans of the Northwest certainly possessed a less intensified civilization than the semi-patrilineal Kwakiutl. It might also be questioned whether the Navajo and Apache should be reckoned as more advanced than the Pima and Yuma. But in each case the quickening hearth of a civilization, the center from which it obviously radiated, the focus at which it reaches its brilliance, is among matrilineal peoples; the penumbra in which the civilization fades out, or indeterminately merges into another, is prevailingly patrilineal. The Tlingit, Haida, and Tsimshian form, or at any rate include, such a focus for the whole Northwest, the Pueblos for the Southwest, in the opinion of every student who has concerned himself with such distinctions. In the East, the Iroquois are ranked above the Algonkians, on the basis of their general culture, and the Muskhogean above the Siouan tribes. If any one holds a contrary opinion, he has not made himself heard. Even the subsidiary and isolated matrilinear canton on the edge of the great Eastern area confirms the rule. It comprises two of three "village Indian" tribes in the nomadic hunting area of the Plains, plus only an established offshoot, the Crow, of one of them; while the third of the town-dwelling, agricultural tribes, the Arikara, are a historically recent branch from the half nomadic Pawnee.

5. By the same scale, the non-exogamic area includes not a single tribe of distinctly advanced or complex culture, no hearth or focus of a type of civilization, but is inhabited over its several millions of square miles wholly by peoples universally characterized as backward in comparison with the bulk of those in the exogamic areas. The Eskimo indeed excel in mechanical inventions; but the remainder of their civilization is certainly of the meagerest; and the same is always stated of the

tribes of the interior of northern Canada, of the intermountain Plateau and Great Basin, of California, and of Texas. Where something of a local civilization has sprung up in these regions, it is invariably a reduced sub-type of a greater civilization centered in an exogamic area; as the culture of the non-exogamic Yurok of northwestern California is clearly only built upon a three or four times filtered leakage from the general Northwest coast culture whose quality is intensest among the Haida.

If these five masses of facts mean anything, they mean this:

So far as the phenomena of what it is customary to call social organization or social institutions in native North America relate to other phenomena, they show an overwhelming connection between lack of exogamy and of unilateral descent on the one hand and the most backward state of civilization on the other; between exogamy and unilateral descent and more intricate and richer civilization. Within the confines of exogamy, patrilineal reckoning prevails among the less advanced nations, and every important acme of civilization is situated among matrilineal peoples. The inference seems inevitable, that so far as North America is concerned, the original or early type of culture possessed no definite exogamic institutions and counted descent bilaterally or only in connection with property and rank. In those regions in which civilization progressed, exogamy grew up, but first with patrilineal descent; where it culminated, it became matrilineal. What is more, this process was independently repeated—certainly in the Northwest as compared with the other areas, possibly in the Southwest and East, not impossibly twice within the East. There is no need of talking in this connection of what are often called "laws": but a positive drift, for a long period and in a positive direction, is manifest.

There is not the least intent to maintain that this indicated development is a sequence of stages having abstract validity, or that it was being passed through by each and every American nation. It must be admitted that we can not positively affirm that any given tribe that is now matrilineal was patrilineal twenty generations ago. The evidence does not yet run so specific. It is also extremely likely that some matrilineal tribes have been matrilineal ever since they followed exogamic rules, without ever passing through a patrilineal condition, and that this or that backward patrilineal people took their exogamy ready-made from a matrilineal group but altered its reckoning to conform with the mode of inheritance prevalent among them. In fact I doubt whether either matrilinear or patrilinear descent cuts very deeply into the life of any American nation. I am inclined to think that it rests comparatively lightly on the genuinely intrinsic anatomy of their society, and that many of them could change and some perhaps have changed from one reckoning to the other without a very serious disturbance of their institutions. Even exogamy seems to me something that they probably take on or put off rather easily; as indeed I hold the whole scheme of clanship, with Dr. Swanton and others, to be a more formal and superficial thing by far than kinship, from the native American point of view itself. I should be ready therefore to allow of a multitude of individual exceptions, especially as regards literal "priority" or "sequence." But just as I believe that next to nothing can be proved as to the actuality of the undifferentiation-patrilineate-matrilineate development *in any given case*, so I believe that Mr. Hartland, in the present state of knowledge, can prove next to nothing as to the actuality or even probability of any particular exceptions. All that we know much about, or can infer as yet, is that the general broad current of development, and on the whole of sequence, indicates strongly that patrilineal institutions underlie matrilineal institutions in North America, and non-exogamic institutions underlie both.

There are only two other interpretations for this body of coherent facts. One is that the advanced tribes, the producers and moulders of civilization, and therefore of institutions, have regularly been conservative of their primitive state as regards the institutions of marriage and descent alone, and thus have maintained an original maternal exogamy, which the backward and ruder tribes once also shared, but in process of time either altered to its patrilineal form or lost altogether. This explanation is extremely unlikely, in the arbitrariness of its supposition that a civilization, or rather several of them repeatedly, should progress along all lines but one, and that this side of life should proceed to achieve new phases only among peoples whose culture in all other respects developed slowly or not at all.

The remaining interpretation is that the whole history of American civilizations since an early period has been one of degeneration, the active and preponderating nations alone supporting their original laws of descent with vigor and success. No one would seriously maintain such a view, I think; but if so, and it were establishable, the victory for the priority of matrilinear institutions would be sterile, since the priority would be one of bare time only, and as regards degree of development patrilineal and bilateral reckoning would remain accompaniments of stunting and lack of resource.

In fine, the correlation between matrilineal institutions and progressive, advanced, and productive types of civilization in North America is too complete, positively as well as reversely, to allow of any conclusion but that in some manner, and for whatever causes, they are definitely connected.

This, then, is what a reconsideration of the facts leads to—a reconsideration guided by the desire to express from the mass of phenomena their vital sap. What is more important, the method by which this interpretation is reached is one of determining the relations of as many facts as possible. Like phenomena are associated; they are set in a relation of geographical space—the dimension which is to the ethnologist not indeed the substitute for time but the first step toward the attainment of that element; and the social phases of civilizations are considered with reference to their other phases. Now this may not be very deep nor far-reaching science, but it is of the very essence of the method of science in its fundamentals, in that it is orderly, impartial, definite in results, based throughout on phenomena, and coherent.

As opposed to this plan, it is fair to inquire on what method Mr. Hartland's denial of these conclusions rests. He makes no serious use of the primary factor of geography. He attempts even less to correlate social institutions of peoples with other aspects of their civilizations or with these civilizations viewed as such. His plan is one of dissociating phenomena as they actually occur, readily selecting those that he can weave into a fabric and contesting the face value of the remainder. All the groupings of facts here presented, except for a few that have been accumulated since the appearance of his paper, were offered by Dr. Swanton, if not formally then suggestively. That Mr. Hartland completely passes over these striking and pregnant associations of phenomena, indicates that the relations of things as they exist have little meaning to him, at least in the field of ethnology. Since no one can find permanent satisfaction in handling a large body of utterly detached and unassociated phenomena, it is evident that he must be putting them into relation by some other process than that of trying to find the relations as they occur naturally. His interest is in the relation which the phenomena bear, or can be made to bear, to a formula: the temporal and developmental priority of matrilineal over patrilineal institutions.

On the other hand, such a conviction as Dr. Swanton outlined and as is here restated is a formulation rather than a formula. But, however designated, it is certainly the *outcome* of a series of orderings of phenomena; while Mr. Hartland, like his predecessors Bachofen, McLennan, and Morgan, *begins* with a formula and attempts to adjust phenomena to it or interpret them in conformity with it. Dr. Swanton is thus inductive and objective; his opponents', subjective an

The latter method, unless restricted rigidly to tentati evitably involves special pleading. And I believe it can not denied that the bulk of Mr. Hartland's argument is speci Every possible contrary instance is examined, harried by cr tion, confronted by contradictory witnesses, its credibility probability doubted on special or general grounds. Car against Radin, Owen against Michelson. The patrilineal marriage also within the mother's clan—a prohibition "o hensible as a relic of matrilineal reckoning"; but when mentions the exactly corresponding case of the matriline marrying into the father's clan, the case is too lacking in si refute or mention. The Sia are so reduced in souls and nu clans that they must perforce violate their laws of exogamy. where most of the tribes without exogamic divisions

> are found, is preëminently a land "where fragments of forgotten . . . It is not surprising that they display wide divergences from t common to the rest of the continent. It is just the situation in whi of old landmarks might be anticipated, ancient rules would be b tinctions lost.

In other words, because the Sia, after more than three resistance to Spanish and American contact, have shrunk to a point where they no longer can maintain their instit would like, the densely massed Californians, comprisin possibly a fifth, of the inhabitants of the entire continen Mexico, living without legend or trace of migrations in h ignorant until recently even of the existence of the Caucasi like people troubled by no more than border quarrels a revenges, and perhaps the least subject of any American g famine—these Californians, whose avowed rudeness of make them matrilineal when actually they are patrilineal or are according to Mr. Hartland's reckoning not "left out c him but are "taken into consideration"—by the applicat a poetic phrase about forgotten fragments.

The final argument follows naturally. Bloody war feuds; tribes wiped out, banished, amalgamated, or inc disturbing effect of horses and firearms, the influence of m traders, government intervention—all these things hav characteristic of American aboriginal history since the cording to Mr. Hartland.

> **It does not surprise me therefore to read of tribes among which few or no remnants of totemic culture, exogamy, mother-right, or the clan system exist. Nor, in these circumstances, can I draw the inference that they never did exist, as they existed among the more settled peoples of the east and south.**

This is very much as if one were to proclaim a natural law, say of gravitation, and when this proved to hold in only a fraction of known instances, to invoke the will of God, or to point blandly with a sweep of the hand to the circumstance that there existed also such a force as electricity in the world, which without exaggeration could be considered potential; and to conclude that the asserted law must be a correct formulation because these extraneous factors might account for the numerous exceptions to it.

Incidentally Mr. Hartland overlooks that his bloodsheds, wanderings, absorptions, and European pressure have been by far the most marked in the matrilineal and exogamic regions of the east and south.

I submit that his method of argument is not that of an unbiased judge endeavoring to sift the most probable conclusion out of a complex mass of testimony, but that of the attorney seeking judgment in favor of a client.

The procedure of those following the method initiated by Dr. Swanton is to integrate the evidence so far as it may be integrated without forcing, and to accept the results that eventuate—those that seem certain, unhesitatingly; those that attain probability, with reserve, but as at least a temporary gain in understanding. The procedure of those who support matrilinear priority in America must be, I think, and that of Mr. Hartland is, to dislocate and disorganize the evidence, to pull it into unconnected bits, so that those fragments which corroborate the preformed opinion may be accepted without further examination, and the contrary ones assailed, besmirched, doubted, and worried. No one may adjudge himself more impartial than his opponent. But I believe that Mr. Hartland himself will not sincerely deny that Dr. Swanton interprets by associating phenomena as they are given in nature or history, whereas he associates only after dissociating them, thus operating more artificially.

Mr. Hartland's challenge to Dr. Swanton to produce an instance of patrilineal institutions changing to matrilineal can therefore be met best not by pointing to this disputed tribe or that half-known people, or by minute dissection of what so-and-so has alleged about either; but by reminding him that the evidence as a whole indicates the very powerful probability that nearly every nation in North America has reached the matrilineal condition from the patrilineal, the unilateral reckoning from the bilateral, exogamy from a previous non-exogamic state; or at least that the apparent drift, so far back as our mental eyes can at present follow it, has been predominantly in this direction. When he has seriously impaired the inherent integrating basis of this broad and insistent inference, it will be time for his challenge to be renewed.

I should be very sorry if this reply to Mr. Hartland's review should be construed as a nationalistic endeavor, or even a nationalistically colored response, in the international field of science and scientific history. There are American anthropologists that agree with him, and British colleagues who dissent. Dr. Rivers has recently pronounced in favor of several of the contentions here advanced. It is the American Morgan who, while not the originator of the view championed by Mr. Hartland, has been perhaps its most influential propagator. That Mr. Hartland has seen fit to contribute his essay to an American organ can only be received as a compliment by Americans; as his unfailing courtesy of presentation must be genuinely appreciated by everyone. If the present reply seem harsh by contrast, as I fear it may, I can only trust that any excess of this quality may be construed as due to the ardor of a conviction: the conviction that, however slight and remote the issue itself be thought, it involves two deeply opposed methods, whose effects in rendering to the science of man understanding and an influence on the lives of men must be profoundly different.

A. L. KROEBER

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"PARALLEL BETWEEN THE NORTHWEST COAST AND IROQUOIAN CLANS AND PHRATRIES"

IN a short paper which appeared recently in the Anthropologist (vol. 19, no. 3, pp. 403–405) under the above title Mr. Barbeau puts forth the suggestion that "the various phratric and moiety systems, appearing sporadically in many parts of North America, may have had a common remote origin or a single center of diffusion" (p. 405). To be sure, this statement is not made in the form of a dogmatic pronounciation, being merely based upon a "feeling" that such may have been the case.

While there can be no objection to attempts being made to find such a common origin, objections certainly must be raised against the methods by which these attempts are being made and especially against the erroneous manner of arriving at conclusions which are totally contradicted by the comparable data in question. As a matter of fact, an examination of these data gives the impression that the paper under discussion has been wholly mis-named and that its proper title should have been "Lack of Parallel between the Northwest Coast and Iroquoian Clans and Phratries." For, if nothing else, the comparisons instituted by Mr. Barbeau into the clans and phratries of the east and those of the northwest prove just the opposite; that is to say, they prove that similarities and correspondences are lacking.

Let us quote Mr. Barbeau's own words! He says, "There is a *close* similarity *only* in the village or tribal system in both areas" (p. 403). A mighty slim correspondence upon which to base even a *feeling* that the various phratric and moiety systems in North America may have had a common remote origin; provided, such a *close* similarity could be found! But upon what features is this *close* similarity based? We must again let the author speak for himself. He says, "their village communities (Iroquoian and Northwest Coast) were alike in so far as they were more or less permanent complexes of kinsmen and mutually unrelated people" (p. 403). What a tremendously close similarity! And how typical of these two areas only! Or does Mr. Barbeau know of any other village communities in North America that are *not* more or less permanent complexes of kinsmen and mutually unrelated people? And this constitutes Mr. Barbeau's chief correspondence!

Let us now examine the other points! "When we compare," says Mr. Barbeau, "the families, clans, and phratries of the Northwest Coast with those of the Eastern Woodlands, we find some *comparable* features, though much specialized in different directions, and many striking differences" (p. 403). Wherein consist these *comparable* features which, in his own words, are "much specialized" and present "many striking differences"? The only comparable feature brought out by Mr. Barbeau is the fact that in both areas we find a totemic organization. Is this general resemblance sufficient to arrive at such far-reaching conclusions; especially in view of the enormous differences found in the details of the so-called "comparable features"? Mr. Barbeau himself is forced to admit that whereas "among the Tsimshian and neighboring nations of the Coast, the phratries, clans, families, and even houses were totemic units, it was *quite different* among the Iroquoian tribes, where the clan alone was totemic" (pp. 403, 404); furthermore, "while the use of its pictorial or plastic representation as a crest was *quite trivial* among the Iroquoian tribes, it reached an *abnormal development* on the West Coast" (p. 404). In this connection I should like to know upon what data does Mr. Barbeau base his supposition that "the West Coast" represents "an abnormal development"? Moreover, Mr. Barbeau admits that "the most fundamental fact in the Iroquoian social system is the *clan*," whereas "on the Coast the *phratries and families alone* are plainly discernible" where "the clan, in the course of time, gradually loses its individuality in the phratry," while "the Iroquoian clan always retained its marked individuality within the phratry" (p. 404). But any lingering doubts concerning the comparability of the totemic institutions among the Iroquoian and Coast tribes are irrevocably dispelled by Mr. Barbeau's final admission that "the *difference* between the phratries in both areas is *radical*. The Coast phratries are *totemic*, the Iroquoian ones are *non-totemic and purely political*" (p. 404).

I am in no way trying to cast aspersions upon Mr. Barbeau's capability as an ethnologist; he has time and again proven himself an excellent and competent observer of the ethnology of the North American Indians. Unfortunately, he seems to have become inoculated with the *Baccillus Reductionis* like some other of his anthropological co-workers and is trying to arrive at sweeping conclusions which are based upon inadequate.and improperly digested data. He evidently starts out with a preconceived notion and is endeavoring to make the facts fit his idea.

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THE ROUTE OF DE SOTO

THE article on "De Soto's Route from Cofitachequi, in Georgia, to Cosa, in Alabama" by Daniel Marshall Andrews, which appeared in the first number of the present volume of the *Anthropologist* is of particular interest to the writer because its author is familiar with the topographical side of the question, a side which I have not had an opportunity to consider with sufficient care. At the same time I can not help thinking that Major Andrews's estimate of the ability of the Spanish adventurers as travelers is very much too low. At the present time I have not the opportunity to make a detailed statement of my own views on this question, but merely wish to indicate them in a very general way.

All writers are very well agreed regarding the position of Cosa, on Coosa river at the mouth of Talladega creek. With the traditional location of Cofitachequi, however, at Silver Bluff, S. C., I am not in accord. From the De Soto narratives themselves, and from the later reports of the Pardo expeditions I feel satisfied that Cofitachequi was lower down the Savannah, perhaps near Mt. Pleasant. I agree with Mooney in placing Xuala near the head of Broad river. This position is indicated, not merely by evidence presented by Mr. Mooney in his *Myths of the Cherokee*, but by information furnished by certain unpublished Spanish documents. In one of these, now in the Lowery collection, in the Library of Congress, a Spanish navigator, touching at the mouth of the Santee, is told that the trail passing up that stream reached a town called Suala at the foot of the mountains, and there is nothing to show that this trail branched off to the westward higher up. There can be no reasonable doubt that Xuala or Suala refers to the Cheraw or Sara, a Siouan tribe, well known in later times, and there is no other evidence that they lived as far south as Major Andrews places them, entirely beyond the range of the other Siouan tribes in the later historic period. Again, there is no evidence other than archaeological, and that not of a thorough character, that there were Indian tribes on the upper Coosa. There certainly were none there after the period of the first English settlements. On the other hand there were such tribes along the Tennessee, and one of them is called by the very same name as that which the De Soto chroniclers employ for a tribe met by them on the river which they descended, the Tali. There is also good reason to believe that the Coste, Acoste, or Costehe were none other than the Koasati, a band of whom are known to have been on the middle Tennessee at the beginning of the eighteenth century. The final *-he* in the last of these forms may very well be intended for the Koasati plural *-ha*. In my judgment the evidence is strong that De Soto marched north from Cofitachequi, on the Savannah, to the head of the Broad, from there through the mountains to the Tennessee, and down the Tennessee to the big bend where he left it and proceeded south into Cosa. A short time before he reached the latter place he came to a town called Tasqui, and about twenty years later some soldiers of Pardo's army who went from Chiaha to Cosa passed through two towns, one called Tasqui, the other Tasquiqui. Now we know from later maps that the Tuskegee tribe actually lived at one time midway between the bend of the Tennessee and the town of Cosa. Another place mentioned by them is "Olitifar" which would correspond, if my supposition is correct, almost exactly with a later Creek town known as Litafatchi. In fact ethnological and historical data seem to back up the Tennessee route at almost every step. The Coosa theory must be maintained on topographical grounds alone in the face of all other evidence.

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ANCIENT INDIAN FIREPLACES IN SOUTH DAKOTA BAD-LANDS: FACT AND FANCY

It is curious what elegant and imposing structures can sometimes be erected upon a foundation built out of our own fancies. Some twelve years ago an article entitled "Ancient Indian Fireplaces in South Dakota Bad-lands" was published in the *American Anthropologist*, vol. 7, no. 1, Jan.–March, 1905. The article told of the finding of some appearances of charcoal and other burned materials

> pocketed in the side of a canyon wall some 40 feet above the bed of the stream and 8 or 10 feet below the top of the cliff. The crumbling Bad-lands clay had fallen away from the sides and bottom of this fire-place, leaving an urn-shaped mass of burned material to adhere to the side of the canyon wall.

The writer of the article jumps to the conclusion that it is a fire-place and goes on to speculate upon the vast stretch of time since the fire-place was used by some prehistoric race of men and that it has since been covered by slowly built deposits of alluvium which again have been cut away by erosion, thus leaving the "fire-place" once more exposed. He concludes thus:

> The problem is this: At some time in the past these fire-places and deposits of kitchen refuse were made by primitive people who were wont to camp at what was then the superficial level of the country. Since that time the entire basin, covering an area of three by ten miles, has been filled with soft Bad-lands clay, regularly deposited by the action of water in eight or ten distinctly marked strata, some of which are filled with the shells of fresh-water mollusks. After the basin had been filled above the old level, where the ancients camped, to a depth of at least ten feet, erosion began its work, since which time the entire basin of horizontal strata has been cut into gullies thirty to sixty feet deep, so that the present creek with its lateral ravines is that much below the top of the surface which extends from one side of the basin to the other. In this process of erosion these ancient fire-places have been exposed to view.

When I first read that article I did not feel satisfied with the conclusion to which the writer of it leaped, but I had no positive evidence as to what the real truth of the matter was, so I could not offer anything in rebuttal. Since then I think I have by good fortune obtained a reasonable and true explanation of the facts. I have become acquainted with Dr. A. McG. Beede, who has been for many years a missionary priest of the Protestant Episcopal church on the Standing Rock Reservation, which is a reservation of the Dakota nation, most of its inhabitants being Teton Dakotas. He has told me what was told to him in 1905 when he read and translated the article in question to a group of old men of that nation. Dr. Beede says:

I found the article "Ancient Fire-places in South Dakota Bad-lands." I thought it would be interesting to these men, so I read it to them. They listened attentively, but twinkling eyes showed that they did not regard the article as correct. I said, "Tell me about these fire-places, what were they, and how were they made." They hesitated for some time, but conferred together and finally decided to let Black Bull be the spokesman. He spoke about as follows: "A white man is very wise, but an Indian is like a child which has little knowledge. (A twinkle in his eye showed quiet ridicule.) But if a grown-up man gets lost he goes very far from the right way just because he is very strong and can go a long way. But a child which is lost does not go very far astray just because the child has not as much strength as a grown-up man. Thus many white men make many very great mistakes; while Indians do not make such big mistakes.

These places which the white man who wrote this story called fire-places are not fire-places at all. That is one of the little mistakes of this wise white man. They were caches where dried meat and dried fruits were stored away safe for future use. Instead of the ground having grown up over these places as his story says, the Indians dug down into the ground and made these places. That is only a small mistake, of course, but it makes considerable difference." With this the speaker gave a quiet, tolerant smile. Then he continued: "If the white man who wrote this story had known old Indian customs he would not have made the small mistake which he made in his story." This he said with a humorous twinkle in his eye. He went on to say: "In old times the Dakotas never roasted a half of a buffalo or even a big piece of a buffalo in a fire-place. They cut up a buffalo into small pieces by sacred rules, and then these small pieces were cooked in various ways. And the Dakotas never had any such fire-places for cooking meat; but all Indians had such places for caches.

"The place for a cache was chosen, then the excavation was carefully dug, and sometimes stones were placed in the bottom of the excavation. Then dried grass and brush was burned in the excavation slowly and carefully. The heat and smoke from this fire made the earth walls around the excavation dry so the frost and moisture would not travel through and spoil whatever was laid away for food. Then, after the food had been stored away in this place underground, the small opening on the surface of the ground, by which the place was entered, was carefully concealed so that marauders could not find it. This concealment was so perfect that even the men who made it could not afterwards find it unless they knew the directions and the number of paces from each of two visible objects which were secretly marked. I know the places mentioned in the white man's story were caches and not fire-places, though I never saw them."

Black Bull, who had been speaking, was a Santee. The Santee was the easternmost tribe of the Dakota nation, and, until scattered and broken by its troubles in 1862 and 1863 was located in southern Minnesota. That is why Black Bull would not be likely to have seen the identical locality where the caches were which had been called "fireplaces" in the article which he had just heard read, for the place was in the western part of the country of the Teton Dakotas.

When Black Bull had ceased speaking he looked towards Red Hail with a twinkle in his eye as though he thought the best of the joke was yet to come. Red Hail was an old Teton, at that time aged about 71 years. The writer of the present article met him last year, not long before his death, which occurred on July 4, 1917, in his eighty-fourth year. Any one who is acquainted with Indians will understand that the arrangement for Black Bull to give the general discussion leading up to the most critical statement and the reservation of that for another was a piece of staging for dramatic effect. Black Bull could give the general statement of the customs common to Indian tribes, but it was left to Red Hail, a Teton, to speak with authority about the particular matter touching the country of his own tribe. When Black Bull glanced toward him it was Red Hail's cue to take up the story. He said:

> Black Bull has well spoken. I will not say over again what he said. But there is one small word which I will say. (Here he smiled gently.) I was there when those caches were made. It was about 60 years ago. I remember it well. I was there with my father, Supe, and the father of Sitting Bull was there. We made two of these caches, and if they look sharp they will find there are two. The meat may be good yet in the one that is not broken open on the butte, and I wish I had some of it now,

he concluded with a whimsical smile in fond recollection of the dear dead past.

MELVIN RANDOLPH GILMORE

PROCEEDINGS OF THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

Meeting of April 17, 1917

THE 511th regular and 38th annual meeting of the Society was held at the New National Museum on April 17. After approving the reports of the secretary, treasurer and auditing committee the society elected the following officers for the ensuing year:—President, Mr. William H. Babcock; Vice-president, Mr. Francis La Flesche; Secretary, Miss Frances Densmore; Treasurer, Mr. C. N. B. Hewitt; Councillors, Mr. E. T. Williams, Mr. Neil M. Judd, Dr. Truman Michelson, Mr. Felix Neumann and Dr. I. M. Casanowicz.

Memorials to members of the society deceased during the past year were then presented. A sketch of the life of Gen. Ellis Spear, an active member of the society, was read by Mr. Wm. H. Babcock. The memorial to Mr. S. M. Gronberger, an associate member, was prepared and presented by Mr. James Mooney, and that to Mr. J. D. McGuire, an honorary member, was prepared and read by Dr. J. W. Fewkes. Tributes to Prof. Johannes Ranke and Prof. Gustave Schwalbe of Germany, honorary members, were given by Dr. Aleš Hrdlička, and a review of the life and work of Sir Edward Burnett Tylor, a corresponding member of the society, was read by Dr. John R. Swanton, tributes to Dr. Tylor being also given by Dr. Truman Michelson and Dr. Leo J. Frachtenberg.

The delivery of the address of the retiring president, Dr. John R. Swanton, on "Some Anthropological Misconceptions," was postponed to a special meeting of the society to be held on May 1.

Meeting of May 1, 1917

THE 512th meeting of the Society was a special meeting held on May 1, in the Natural History building of the National Museum to hear the address of the retiring president, Dr. John R. Swanton, whose subject was "Some Anthropological Misconceptions."

Dr. Swanton began by calling attention to the cyclic nature of cultural movements and stated that like other beliefs the doctrine of evolution which so dominates the thought of our time is subject to the same law and bound to have its rise, decline, and disappearance as an object of peculiar interest, andthat the truth embodied in it will in time become so axiomatic that no particular attention will be paid it while the chaff will disappear.

Unfortunately when pioneer anthropologists began to apply evolutionary ideas to their science, then in its infancy, they fell into a serious error. They assumed, with some justice indeed, that the existing peoples of the world presented features some more and some less primitive, features which might be arranged into series showing the stages which mankind as a whole had passed through, but in selecting the "most primitive" features they worked on the false assumption that that which was most foreign to the ideas of the society in which they lived, in the cultural center of western Europe, were the most primitive. This resulted in a vast crop of pseudo-scientific evolutionary theories each based on its author's own peculiar understanding of what was more and what less primitive. An assistant source of error was an over earnest attempt to find survivals analogous to the "vestigial characters" of biology in all kinds of cultural features, many of which were not vestigial at all. The speaker referred to several evolutionary theories of this kind, treating at some length those regarding the evolution of totemism from a matrilineal clan system, the evolution of marriage from a primitive promiscuity, and several theories concerning the origin of religion, such as those of Spencer, Tylor, Frazer, and Lang.

Secondly, the author took exception to the extreme uniformitarian attitude taken by certain anthropologists. He called attention to the fact that absolute uniformitariansim is impossible since even the inorganic world is based on discreted molecules, atoms, electrons, etc., while the organic world is based on independent organisms. In the same way when we turn to the culture history of mankind we find that ideas, although progressive, do not roll into consciousness with the even motion of a wheel but come at certain definite times and places.

Along with this extreme uniformitarianism he believed too much stress had been placed on the unconscious or subconscious side of evolution in human institutions. Important as the latter undoubtedly is and much as it is neglected by the man of average intelligence, it acts less toward the development of new institutions than toward the preservation of institutions already in existence, and is accompanied rather by degeneration, or at most imitation, than by absolute origination.

In this connection Dr. Swanton took occasion to criticize a certain type of student who, because he observes the powerful effect of subconscious imitation, assumes that there is an extra-mental current which settles all problems, and looks cynically upon conscious efforts to bring about change. When examined microscopically this seemingly unconscious current would be found to be a resultant of forces, each of which was the decision of some individual or some group of individuals at a definite time and place. He believed that if any of these decisions had been different the stream itself, the course of history, would in some measure have been different.

In the discussion which followed the address Dr. Leo J. Frachtenberg agreed in main with the assertions made by the speaker. He called attention to the fact that the error of particularization is well exemplified in Westermarck's *Origin of Primitive Ethics.* Dr. Frachtenberg expressed the belief that the principle of unconscious evolution should not be underestimated. Dr. Truman Michelson added as another misconception the supposition that the languages of primitive peoples indicate a low mentality, stating that it is possible to express complicated ideas by means of these languages but that the ordinary life of the people does not require such expression. Another misconception, according to Dr. Michelson, is the arranging of languages in a scale of superiority with inflectional languages as the highest point of development.

In closing the discussion Dr. Swanton stated that although it is true that the unconscious element plays a very important part in the evolution of culture its functions are conservative, or, at most, imitative with a tendency toward degeneration, whereas the conscious element is that which creates, that which produces positive advances.

FRANCES DENSMORE, *Secretary*

ANTHROPOLOGICAL NOTES

DR. JOHN P. HARRINGTON, of the Bureau of American Ethnology, returned to Washington October 8, after field work among the Indians of the Southwest covering several years.

DR. TRUMAN MICHELSON, of the Bureau of American Ethnology, has been appointed Professor of Ethnology in the George Washington University. He will also retain his position as ethnologist in the Bureau. The University will offer three courses in ethnology under the instruction of Dr. Michelson: a "General Introduction to Ethnology," "Outlines of North American Ethnology," and "American Indian Languages." Dr. Michelson returned to Washington October 6, from field work among the Fox Indians of Tama, Iowa, the Sauk of Kansas and adjacent parts of Nebraska, and the Potawatomi of Kansas.

DR. ALEŠ HRDLIČKA, of the U. S. National Museum, returned in August from field work devoted largely to a study of the mountaineers of Tennessee. He also visited the Shawnee and Kickapoo Indians of Oklahoma.

MR. PHILIP AINSWORTH MEANS was appointed in August, Honorary Collaborator in Archaeology in the U. S. National Museum. Early in October he left Washington to undertake archaeological researches in the northernmost provinces of Peru.

MR. LESLIE SPIER, of the American Museum of Natural History, is in the Southwest, continuing the archaeological investigations begun by him last year.

THE ANTHROPOLOGICAL SOCIETY OF PHILADELPHIA resumed its activity on the evening of November 17 when a paper was presented on Egyptian life by the President of the Society, Dr. W. Max Müller. With Mr. E. P. Wilkins as Secretary and twenty members the Society started its fourth year.

THE GEORGE G. HEYE NORTH AMERICAN INDIAN COLLECTION which has been exhibited for the past eight years in the Museum of the University of Pennsylvania, has been withdrawn and will be exhibited in the new Museum of the American Indian in New York which has been founded by Mr. Heye.

DR. G. B. GORDON, the Director of the University Museum, is away on a six months' leave of absence and Dr. Farabee has been appointed Acting Director in his place.

MR. H. U. HALL, the Assistant Curator of the Section of General Ethnology, left the Museum on July 15 in order to give his services to the country. He is now with the Second Pennsylvania Field Artillery at Camp Hancock, Augusta, Ga.

MR. B. W. MERWIN, the Assistant Curator in the American Section, left the Museum on September 18 for military service. He is stationed at Macon, Ga.

THE UNIVERSITY OF PENNSYLVANIA'S second series of lectures on "The History of Religions" opened November 19 with a lecture by Frank G. Speck on "Primitive Religion." The following men will lecture during the series: W. Max Müller, Morris Jastrow, Jr., James A. Montgomery, Franklin Edgerton, Roland G. Kent, Walter W. Hyde, George D. Hadzsits, and Amandus Johnson.

THE COMMERCIAL MUSEUM OF PHILADELPHIA, under the direction of Mrs. L. L. W. Wilson, has concluded its third summer of excavations at Otowi, Bandelier National Monument, New Mexico, excavating all the rooms of North, South, and East Houses of Great Otowi, all of the rooms of the East House, Little Otowi, in addition to several rooms in the West House and many single rooms and one kiva in the seventeen small-house ruins of Little Otowi. The caves in the immediate neighborhood have been explored. The whole region has been mapped. Many interesting discoveries were made, and considerable material obtained.

PROFESSOR ALFRED M. TOZZER, of the Department of Anthropology of Harvard University, Secretary of the American Anthropological Association, has been commissioned a captain in the Flying Corps. He is stationed at Denver, Colorado, where he is engaged in making the non-physical tests of recruits for the air service.

THE NEW STATE MUSEUM at Santa Fé was dedicated November 24–28. Various museums and learned societies were represented by delegates who presented papers at the several sessions. Trips were made to neighboring points of ethnological and archaeological interest.

DR. W. C. FARABEE is acting as Secretary of the American Anthropological Association in the absence of Professor Tozzer.

MR. JOSEPH T. GOODMAN, editor, writer, and archaeologist, died on October 1, in San Francisco, California.

REV. HORACE EDWIN HAYDEN, who has been associated with the Wyoming Historical and Geological Society for the past twenty-five years, died at his home in Wilkes-Barre, Pennsylvania, on August 22, 1917.

MR. CHRISTOPHER WREN has been elected to fill the position in the Wyoming Historical and Geological Society left vacant by the death of Rev. Horace Edwin Hayden.

DR. SALVADOR DEBENEDETTI has been appointed Director of the Ethnographical Museum of the Faculty of Philosophy and Letters in Buenos Aires, Argentina, filling the vacancy caused by the death of Dr. Juan B. Ambrosetti.

TEOBERT MALER died on the twenty-second of November, in his seventieth year, at Merida, Yucatan. Mr. Maler, who was of Austrian descent, came to Mexico with an officer of Maximilian's army when a lad of thirteen. He soon became interested in the antiquities of Middle America, to the study of which he devoted most of his life.

L'ÉCOLE D'ANTHROPOLOGIE DE PARIS opened its 42d year on November 3, with courses offered as follows:

1. R. Anthony. Development of the brain in man and the apes.
2. L. Capitan. Art and architecture during the neolithic and protohistoric periods.
3. G. Herve. Ethnology and ethnography in France during the XVIIIth century.
4. P. G. Mahoudeau. The precursors and the authors of evolution: Buffon, Lamarck, Darwin.
5. L. Manouvrier. Ethnic psychology.
6. A. de Mortillet. Burial customs among ancient and modern primitive races.
7. G. Papillault. Psycho-social values and sophisms.
8. F. Schrader. Geographic causes of rapprochement and differentiation among human groups. Evolution of the old world.
9. J. Vinson. Primitive languages, popular language, folk-lore.

In addition there are two short courses of eight lectures each on:

1. The survival of primitive industries, by D. Bellet;
2. Falsehood from the viewpoint of anthropology and criminology, by Paul Boncour.

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